ESSAYS IN APPLIED MICROECONOMICS

TENG LI

(*B.S., Sun Yat-sen University, China*)

A THESIS SUBMITTED

FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

DEPARTMENT OF ECONOMICS

NATIONAL UNIVERSITY OF SINGAPORE

2017

Supervisor:
Professor John C. Ham

Examiners:
Dr. Eric Fesselmeyer
Dr. Slesh Anand Shrestha
Assistant Professor Kim Seonghoon, Singapore Management University

Declaration

I hereby declare that this thesis is my original work and it has been written by me in its entirety. I have duly acknowledged all the sources of information which have been used in the thesis.

This thesis has also not been submitted for any degree in any university previously.

Signed: Teng Li

Date: **7 Jun, 2017**

Acknowledgements

Upon the submission of my thesis, I would like to take this opportunity to look back on my Ph.D. career and thank all those great professors, classmates, and families who have helped and supported me along this fulfilling road.

First and foremost I would like to thank my supervisor John C. Ham. It has been a great honor for me to be his Ph.D. student. He has guided me, both consciously and unconsciously, toward a deeper understanding of academic research. Without the guidance of him, I cannot imagine how I can get through all the difficulties during the years of my Ph.D. studies. I sincerely appreciate all his contributions of time, ideas, and enthusiasm to make my Ph.D. experience productive. He is always willing to provide patient explanations, helpful discussions, constructive suggestions, and positive advice for my questions and the problems I encountered.

Next, I would like to express my sincere gratitude to Professor Yongheng Deng. He has supported me by offering excellent discussions and brilliant suggestions on my work. More importantly, I am so fortunate to meet him when I need a mentor. His encouragement helped me to live through the most difficult time. Additionally, I thanks Professors Sumit Agarwal, Cristian Badarinza, Alice Chen, Yuyu Chen, Junhong Chu, Olivier Deschenes, David Figlio, Yuming Fu, Rema Hanna, Bhashkar Mazumder, Ivan Png, Wenlan Qian, Daniel Rees, Slesh Shrestha, Aloysius Siow, Changcheng Song, Junjian Yi, and Klaus Zimmermann for providing helpful suggestions and discussions on my thesis.

I am also grateful to all my friends and classmates I met in NUS. They provided helpful academic discussions on my studies. Their selfless assistance, experience sharing, and emotional support enrich my Ph.D. career over the four years.

I also acknowledge the financial support of the Musim Mas Dissertation Award from the Musim Mas Group, which helped me to accomplish my second chapter of the thesis.

Last but not the least, I would like to give my deep and sincere gratitude to my family for their continuous and unparalleled love, support and unwavering belief in me. They make me enjoy my life and concentrate on my research. I could not have started or finished my thesis without their substantial and spiritual support. This thesis is dedicated to them.

Contents

Summary

My thesis consists of three chapters, which study the effects of environmental factors (e.g., temperature and air pollution) on human well-beings and consequences of environmental policies.

In the first chapter, we examine the effects of high temperatures during pregnancy on birth weight and later outcomes for Chinese adults.[1] We find that exposure to hot weather triggers lower birth weight, and, in adulthood, non-negligible adverse effects on educational attainment, cognitive abilities, and height. The impacts are concentrated in the second trimester. Back-of-the-envelope predictions suggest that by the end of the 21st century, ceteris paribus, newborns on average will weigh 10-50 grams less, and losses in education years and height will be 0.10-0.40 years and 0.14-0.55 cm, respectively, caused by global warming. Furthermore, we also find that the adverse effects of high temperatures are less likely to be caused by income effects.

The second chapter examines the impacts of air pollution on criminal activities by exploiting three dimensions of variations in a rich quasi-experiment: the NO_x Budget Trading Program. This program has been well documented to decrease NOx emissions and ozone concentrations in participating states. Employing a triple-difference estimator, we find robust evidence that the cap-and-trade market statistically significantly reduced violent crimes in participating states, whereas property crimes were less affected.

[1] This chapter is co-authored with my classmate Zihan Hu.

Instrumental variable estimates suggest that lowering pollution emissions may play an important role in reducing violent criminal behaviors.

The last chapter analyses how housing markets were affected by the NO_x Budget Trading Program.[2] Using a difference-in-differences method, we find that house prices in the NBP areas with low manufacturing intensity shifted up, consistent with the prediction from hedonic theory, but for high manufacturing intensity areas, housing markets were weakened. Although the emission market provided a market-based solution to abate air pollution, our study presents evidence on its unintentionally impact on wealth redistribution.

[2]This chapter is co-authored with Prof. Sumit Agarwal and Prof. Yongheng Deng.

List of Tables

List of Figures

Chapter 1

The Long-term Effects of High Temperatures during Pregnancy—Evidence from China

1.1 Introduction

The rise in of greenhouse gas concentrations caused by anthropogenic emissions is associated with global warming. By the end of the 21st century, average global temperatures are expected to be 0.5°F to 8.6°F higher than 2000 levels (Intergovernmental Panel on Climate Change 2013).[1] Estimating the costs related to climate change is of great importance

[1] Based on predicted data by the National Aeronautics and Space Administration (NASA), Figure A1.1 displays a vivid example, which contrasts the global daily maximum near-surface air temperature on the 1 July 2000 (panel a) and 2100 (panel b). It shows that in 2100 the high temperatures in some places (e.g., North Africa and Arabian Peninsula) could approach 320 K (around 116°F), assuming that greenhouse gas

for policy makers who seek to design rational climate-change-mitigation policies.

A developing literature in economics finds that hot weather during pregnancy causes negative effects on birth outcomes (Deschenes et al. 2009; Barreca et al. 2015). An important question that has yet to be answered is whether the adverse effects of hot weather during the prenatal period on birth outcomes are further related to adult welfare outcomes, and to what extent. To fill this gap, in this study we examine the effects of high temperatures on educational attainment, cognitive abilities, and height for Chinese individuals born in rural areas between 1950 and 1994.[2]

Combining individual characteristics from the China Family Panel Studies (CFPS) with weather information, we find that hot weather during pregnancy triggers significant reductions in adult welfare in multiple dimensions. Specifically, adults who experienced one standard deviation more high-temperature days (around 34 days) during the prenatal period attain 0.29 fewer years of schooling, are 7.59% and 4.83% standard deviations lower for word- and math-test scores, respectively, and are 0.40 cm shorter.[3] In addition, the impacts seem to be concentrated in the second trimester. High temperatures in the first and third trimester do not have statistically significant effects. Furthermore, we find a large effect on birth weight for high temperatures during pregnancy. A one standard deviation increase in the number of high-temperature days leads to a loss of 73.86 grams

emissions will peak around 2100.

[2] Weather information pre-1950 is not available.

[3] We also find that the effect of high temperatures during the gestational period on personal annual income is negative but not precisely estimated.

of birth weight (12.35% standard deviation).[4] Given the strong relationship between birth weight and children's development and adult outcomes (e.g., Behrman and Rosenzweig 2004; Black et al. 2007), this finding suggests that the adverse high-temperature effect on birth weight could be the channel for high-temperature effects on adult outcomes.

Such effects, however, have not been taken into account when calculating the costs of global warming. Based on climate projections provided by the National Aeronautics and Space Administration (NASA), we perform back-of-the-envelope predictions for birth and adult outcomes of individuals born in rural areas of China in 2100. Compared to those born in 2000, *ceteris paribus*, babies born at the end of the 21st century will weigh 10-50 grams less on average. Further, in adulthood the losses in educational attainment and height will be 0.10-0.40 years and 0.14-0.55 cm, respectively.[5]

We propose several hypotheses that may explain why hot weather affects birth outcomes. The first draws on evidence from medical research (see Strand et al. 2011 for a detailed review). Due to the extra physical strain, a pregnant woman is more susceptible to ambient heat stress. By influencing the physical health conditions of pregnant women, high temperatures play an important role in fetal size and development.[6] Moreover, Berry et al. (2010) argue that high temperatures could affect mental health directly by exposing people to trauma or indirectly by physical health. Above a threshold of 26.7°C

[4]The effect on birth weight for individuals born in urban areas is not statistically or economically significant. See results section for details.

[5]We acknowledge that our predictions strongly rely on the assumption that all other related factors will remain constant. See discussion section for details.

[6]For details, see discussion section.

(around 80°F), Hansen et al. (2008) observe a positive relationship between temperature and hospital admissions for mental and behavioral disorders. Economists recently show a few pieces of evidence that parental mental problems could cause low birth weight, e.g., Duncan et al. (2016) and Persson and Rossin-Slater (2016).

Another possibility, referred to as the income effects, is that high temperatures affect household resources and nutrition for pregnant women by influencing crop yields—the main income source in rural areas (Hollinger and Angel 2009; Schlenker and Roberts 2009; Burgess et al. 2011). For instance, Schlenker and Roberts (2009) find that temperatures above about 85°F cause damages to corn and soybeans yields. Three pieces of evidence suggest that income effects are unlikely to be driving our results. First, areas with high proportions of heat-tolerant crops (corn and sugarcane) do not significantly mitigate estimated temperature sensitivity during pregnancy. Second, simultaneously controlling for weather conditions during (a) the last year growing season before birth and (b) the prenatal period, we find that the former has no significant effects on birth weight or adult outcomes. Third, we find that the adverse effects are concentrated on the second trimester. Income effects can explain this pattern only if there are large overlaps between growing seasons and the second trimester. However, birth months in our sample are not concentrated in any certain seasons, and the high-temperature days across trimesters are similar to each other. Based on these results, we conclude that the adverse effects of high temperatures during pregnancy are less likely to be triggered by income effects.

An additional hypothesis is called "behavioral effects", i.e., hot weather may induce changes in daily activities of pregnant women and further affect birth outcomes. For instance, Herman (1993) documents that ambient heat could reduce appetite and alter food selection, which are critical for fetal development (Figlio et al. 2009). As the data do not contain information on individual activities during pregnancy, we cannot directly rule out the behavioral effects. But we find a piece of suggestive evidence which behavioral effects may hardly explain, i.e., the adverse effects of high temperatures during pregnancy are concentrated on the second trimester. It is hard to imagine that pregnant women change food intake and selection only at the second trimester as a result of ambient heat.

The current study contributes to the literature in several ways. First, our paper provides the first evidence for the long-term persistent effects of high temperatures during the prenatal period on adult outcomes, along with two working papers by Carrillo et al. (2015) and Isen et al. (2015). Deschenes et al. (2009) use data from 49 states in the U.S. and find that exposure to days above 85°F during pregnancy has a moderate negative effect on birth weight. Whether the effect is further related to adult outcomes (e.g., human capital, physical conditions, etc.), as the authors claim, is an important—but unanswered—question. Using a developing country context—China, our study shows that adults' educational attainment, word- and math-test scores, and height are negatively affected by hot weather in *utero*.

Evaluating the adverse impacts of hot weather is highly relevant in China, especially for about 600 million rural residents, because they in those areas have limited access to avoidance behaviors such as air conditioners (Brooks et al. 2005; Feng et al. 2010). For instance, as late as 2009, each household in rural China owned only 0.12 air conditioning units (China Statistical Yearbook 2010). In contrast, around 87% households in the United States were equipped with at least one air conditioning unit in 2004 (Barreca et al. 2016). The limited access to avoidance behaviors may amplify the impacts of high temperatures in rural China.

To compare, Isen et al. (2015) employ the U.S. data and find that hot weather during pregnancy reduces annual income but does not affect educational attainment. Another study by Carrillo et al. (2015) shows that 1°C increase in average temperature during pregnancy reduces income and education attainment in Ecuador. Additionally, besides educational attainment and income, we also examine the effects of high temperatures on more dimensions of adult outcomes, including cognitive abilities and physical conditions. Furthermore, our study provides a detailed discussion of the potential mechanisms that explain why hot weather affects birth outcomes. Several pieces of evidence support that income effects are not the key channel.

Second, our study contributes to a growing literature which studies the relationships between early life conditions and later outcomes (see Currie and Almond 2011 for a comprehensive review). Several influential studies have examined the consequences of early life shocks, such as the influenza pandemic (Almond 2006), Chernobyl disaster (Almond et al. 2009), and hurricanes (Currie and Rossin-Slater 2013), and find that such shocks have persistent and profound effects on well-being in later life. The unusual nature of these events, however, raises concern about the generalizability (Maccini and Yang

2009; Almond and Mazumder 2011). Recent studies start to examine the effects of typical events in early life, such as rainfall (Maccini and Yang 2009), alcohol availability (Nilsson 2014), and nutrition restriction caused by Ramadan (Almond and Mazumder 2011). We complement this strand of literature by investigating the effects of high temperatures during pregnancy—another typical variation in early life—on birth weight and later outcomes.

Lastly, from a broader perspective, our findings may add to the literature that explains the positive correlation between latitude and economic development. Many scholars have found convincing evidence that economic activities are correlated with geography indirectly through historical channels (see Wacziarg and Spolaore 2013 for a review). Some studies, however, show alternative direct explanations for such phenomena, e.g., a high burden of disease and the pests and parasites that thrive in hot climates. Based on our findings, we may provide another explanation, i.e., high temperatures affect newborn endowment, and further human capital, which is crucial for economic development.

Section 2 describes our data and variable definitions. Section 3 introduces the econometric approach. Section 4 presents the main findings, while Section 5 discusses the possible channels behind the impacts, implements robustness checks, and predicts the effects of global warming. We conclude in Section 6.

1.2 Data and descriptive analysis

1.2.1 Data source

Welfare outcomes and birth weight. Our major source of data on adult outcomes is the China Family Panel Studies (CFPS) 2010, a nationally representative, annual longitudinal survey of Chinese communities, families, and individuals.[7] The studies were launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University and cover 25 provinces that represent 95% of the total population of China (Xie 2012).[8]

Many adult outcomes are included in the survey—e.g., years of schooling, word- and math-test scores, height, and annual income. In specific, the interviewers investigate individuals' education levels, which consist of eight categories, i.e., illiterate/semi-illiterate, primary school, junior middle school, senior middle school, junior college, college, master's degree, and doctoral degree. The year of schooling is then imputed on the basis of the education levels by the survey. The two test scores—word- and math-test scores—reflect individuals' cognitive abilities. In the word and math tests designed by the CFPS, respondents are required to read as many Chinese characters as possible and to solve basic math questions, including arithmetic operation, exponents, logarithms, permutation and combination, etc.[9] For the sake of interpretation, the scores have been standardized in our

[7]The CFPS is a biennial survey, designed to be complementary to the Panel Study of Income Dynamics (PSID) in the United States.

[8]The 25 provinces are Beijing, Tianjin, Hebei, Shanxi, Liaoning, Jilin, Heilongjiang, Shanghai, Jiangsu, Zhejiang, Anhui, Fujian, Jiangxi, Shandong, Henan, Hubei, Hunan, Guangdong, Guangxi, Chongqing, Sichuan, Guizhou, Yunnan, Shaanxi, and Gansu. Figure A1.2 in the appendix shows their geographic distribution.

[9]See the CFPS (2010) user's manual for a detailed description.

empirical analyses. Height reflects individuals' physical conditions. The annual income is the interviewees' total personal income in 2010.[10]

The data set provides ample information on demographic status as well, such as date of birth (month and year), gender, race, county of birth, birth order, number of siblings, and parental characteristics—e.g., age, educational attainment, etc. Based on the date of birth, we define each individual's prenatal period as nine months before the birth, or around 270 days in total.[11] The whole period is typically divided into three trimesters. Socio-economic information may help us capture family heterogeneity across different areas with different climates.[12]

In addition, relying on interviewees' own birth weight data, we examine the channel for high-temperature effects on adult outcomes. During the survey, interviewees were required to report their own birth weight if they remembered. It is a custom in China that doctors tell the parents their newborn's birth weight. Thus people could know their own birth weight from their parents. In the data set, a limitation is that only one third of the

[10]In our sample, there are many cases that several members in one family are all agricultural workers. Therefore, it is difficult to measure the personal income precisely. We acknowledge this problem and treat our findings about the effects on personal income as only suggestive evidence.

[11]The prenatal period is inevitably measured with error, as the exact birthdate and gestational length are not available. The nine-month gestation period is supported by Deschenes et al. (2009). In addition, Patel et al. (2004) find that the median gestational age at delivery is about nine months in Asians. But we acknowledge that high temperatures could also increase the probability of preterm birth (Schifano et al. 2013). To check the sensitivity of our results to the defined gestational length, we switch the nine-month period to eight months in regressions. Table A1.1 presents the effects of high-temperature days during the eight months before birth, and we note that most estimates remain stable.

[12]For instance, Buckles and Hungerman (2013) find that the relationship between season of birth and later outcomes is driven by maternal characteristics. In analysis, we also explore the differential effects of high temperatures during pregnancy by parents' characteristics. But the results indicate that the effects do not vary by parents' characteristics.

interviewees remembered their birth weight.[13] We then examine the correlations between birth-weight-missing indicator and demographic status. It shows that individuals whose parents are better educated have a higher probability to remember their birth weight. This implies that the adverse effects of high temperatures during pregnancy on birth weight may be underestimated in this study, because the effects are likely to be larger for individuals with worse family background.[14]

Weather data. Weather data are from the China Meteorological Administration and the National Oceanic and Atmospheric Administration (NOAA) and include 1,509 different weather stations across China. The data set contains daily maximum and minimum temperature and precipitation. High-temperature days are defined as the number of days with daily maximum temperatures higher than 85°F.[15] To ensure the accuracy of the weather readings, our key variable is defined as the average of the number of high-temperature days of all the weather stations whose distance to the county's centroid are less than 80km and that do not vary in elevation by more than 200 meters. Using alternative distance thresholds, such as 60 km and 100 km, does not change our main results.[16] Hereafter, we refer simply to "high-temperature" or "hot-weather" days. In our sample, a representative rural pregnant woman is exposed to about 48 hot-weather days out of

[13]We acknowledge that there is a possibility of recall bias on the birth weight variable among those who report their birth weight. But as the errors are less likely to be correlated with our explanatory variable, they should not affect our estimates.

[14]In Table A1.2, we further examining the adverse effects of high temperatures during pregnancy for the two group individuals—with and without birth weight data. And we do not find any statistically significant differences. See details in the main results section.

[15]To test the sensitivity of the estimates to the temperature threshold, we apply different thresholds, ranging from 70°F to 90°F. See the main results section for detailed analysis.

[16]Corresponding results are summarized in Table A1.3 and A1.4 in the appendix.

nine months of pregnancy. Other meteorological controls include the number of low-temperature days and total precipitation during pregnancy. Low-temperature days are defined as the number of days with daily minimum temperatures lower than 32°F.

In our analysis, we restrict our sample to individuals born in rural areas, which comprise 84.05% of the original CFPS sample. Since individuals in rural areas in general work outside frequently and have limited ways to avoid ambient heat, such as air conditioners, they are more likely to suffer from hot weather. Furthermore, observations without exact information on birth place are excluded. The remaining sample contains 10,685 individuals in 143 counties across 25 provinces (see Figure A1.2). Sample statistics are summarized in Table A1.5.

1.2.2 Descriptive regional patterns

If ambient heat stress during the prenatal period is an important determinant of welfare outcomes, we would expect that individuals in warmer regions have worse adult outcomes on average. In this subsection, we first depict the relationships between temperature and adult outcomes across provinces. Next, we examine the correlations between temperature and birth weight across provinces, as the high-temperature effects on adult outcomes are possibly caused by the high-temperature effects on birth weight.[17]

Welfare outcomes against temperature. Panels (a) through (d) in Figure A1.3 plot

[17]Importantly, the correlations depicted below only serve as the motivation for this project. They do not imply any causal interpretations. They could be explained by other factors. For instance, northerners in China are on average taller than Southerners, and this may be explained by differences in cuisine or genetics, rather than high temperatures.

the mean years of schooling, word- and math-test scores, and height, respectively, against the number of hot-weather days (>85°F) for a representative gestational period across provinces.[18] Relative to the southern provinces (blue circle markers in Figure A1.3), provinces in the north (red square markers) suffer hot weather less frequently.[19] The four figures indicate that hot weather during pregnancy may be further related to welfare losses in adulthood. Specifically, Panels (a)-(c) display interesting regional patterns that adults born in warmer places (lower latitudes in general) have fewer schooling years and lower word- and math-test scores. Panel (d) shows the same regional pattern that the warmer the area, the greater the loss in height. This phenomenon in China—the higher the latitude, the taller the people—is also documented by Buxton (2013). Our findings suggest that low birth weight caused by climate may explain this geographical distribution of height to some extent.

Birth weight against temperature. In Figure A1.3, Panels (e) and (f) plot mean birth weight and low-birth-weight likelihood (<2,500 grams, LBW hereafter) for each province against the number of high-temperature days in a representative gestational period. The regional pattern of birth weight is striking and consistent with that of adult outcomes: Typically, babies born in the southern provinces gain less weight and are more likely to suffer from LBW. For perspective, Guangdong, Guangxi, and Fujian provinces, which are located in the southest China, are the warmest areas of China, with around 90 days with

[18] Beijing is excluded from these panels, since the average years of schooling and word- and math-test scores for individuals in Beijing are far beyond those in other provinces.

[19] We use an official geographical dividing line—the Huai River-Qin Mountains—to define northern and southern China provinces.

a maximum temperature higher than 85°F in a typical year. Compared to a representative baby in China, babies born in these three provinces weigh less by 3.6%, 8.7%, and 11.4%, respectively.

1.3 Empirical framework

To exploit how high-temperature exposure during pregnancy affects adult outcomes, we employ the following specification:

$$Y_{ijmt} = \beta HighTemp_{ijmt} + W'_{ijmt}\gamma + X'_i\delta + \mu_j + \lambda_t + \eta_m + \theta_{pt} + \epsilon_{ijmt}. \qquad (1.1)$$

Here, *i* references individual, *j* represents county, *p* indicates province, and birth month and year are denoted by *m* and *t*, respectively. The dependent variables, Y_{ijmt}, are adult outcomes, including schooling years, standardized word- and math-test scores, height, and annual income.[20] The variable of interest in Equation (1) is $HighTemp_{ijmt}$, the number of hot-weather days during the gestational period. Other meteorological factors (W_{ijmt}), such as the number of cold-weather days and total precipitation during the gestational period are controlled. We add a vector of individual characteristics, X_i, including gender, race, birth order, number of siblings, and parental age at delivery and educational attainment, to capture individual heterogeneity. To account for any time-invariant county-level factors, we control for μ_j, a county fixed effect. λ_t and η_m represent birth year and

[20]We use this specification to examine the effects of high temperatures on birth weight as well.

month fixed effects, capturing common shock over years and seasonality patterns.

In addition, the province-year fixed effects, θ_{pt}, are added. The two-way fixed effects could capture the nonlinear changes in the determinants of human capital formation. As during our sample period, China enacted several policies that likely created nonlinear province-specific differences over time, for instance, the collectivization of land (late 1950s), the Three Years of Great Chinese Famine (1959-1961), the Cultural Revolution (1966-1976), and 9-year Compulsory Education (1986). Concerning the regional and year specific seasonality, we also add the year-month and province-month fixed effects as a robustness check in appendix. The results are shown in Table A1.6. The estimates are reasonable robust but are not as precise. ϵ_{ijmt} denotes an idiosyncratic random error term. To allow for potential temporal and spatial autocorrelations, standard errors are clustered at the county level.[21]

Furthermore, if families can non-randomly sort across geographic regions, then it is unclear whether a causal interpretation can be implied. In other words, richer families may have sorted to areas that produce more abundant crops, which are also areas where there is less exposure to heat in *utero*. In such a case, less exposure to ambient heat during pregnancy is correlated with better adult outcomes. However, before 1990s in China, people are severely restricted on migration and relocation, especially for rural residents

[21] We also check the robustness of inferences. Our regression sample includes 10,685 individuals born in different counties, years, or months. We randomly assign individuals born in a placebo time and place, and re-estimate Equation (1) 1,000 times. If the standard errors were consistent, the rejection rate of the null hypothesis of no effect should be around 5% of the time when the threshold for the absolute t-statistic is 1.96. As shown in Figure A1.4, cases with an "effect" significant at the 5% level are around 5% of all placebo estimates. The appendix also reports standard errors that allow for autocorrelation within province. As shown in Table A1.7, the inferences remain unchanged.

(Chen and Zhou 2007). The residence registration system, called "Hukou" system, is the main limitation on regional mobility. To further address the concern, we replace the province-year fixed effects with county-year fixed effects in the main specification to check the robustness of our results.

To further validate our main specification, we regress personal characteristics on high-temperature days during pregnancy including the set of fixed effects in the main specification. If high-temperature days are not associated with the observable characteristics, it would support the exogeneity of high-temperature days in our main specification. The results in Table A1.8 show that all coefficients of high-temperature days are far from statistically significant at the traditional level, indicating that the high-temperature days are not correlated with any observable characteristics conditional on the set of fixed effects in our main specification. This finding suggests that the high-temperature days in our main specification are exogenous. Additionally, most coefficients on low-temperature days and precipitation are also not statistically significant.[22]

As suggested by the epidemiological literature, high-temperature exposure in different trimesters may have heterogeneous effects on birth weight and then further on adult outcomes. In the following specification, we allow for such heterogeneity:

[22]In contrast, Wilde et al. (2016) find that because of fetal selections in Sub-Saharan African, hot weather around the time of conception induces better educational attainment later in life and lowers child mortality.

$$Y_{ijmt} = \sum_{T=1}^{3} \beta^T HighTemp_{ijmt}^T + \sum_{T=1}^{3} \gamma^T W_{ijmt}^{\prime T} + X_i^{\prime}\delta + \mu_j + \lambda_t + \eta_m + \theta_{pt} + \epsilon_{ijmt}, \tag{1.2}$$

where $HighTemp_{ijmt}^T$ denotes the number of hot-weather days in each trimester. $T = 1$, 2, and 3 denote the first, second, and third trimester, respectively. W_{ijmt}^T consists of the number of cold-weather days and total precipitation in each trimester. The other notations are the same as those in Equation (1).

1.4 Main Results

This section reports estimates of the effects of ambient heat stress during pregnancy on later-life well-being, such as education years, cognitive abilities, height, and annual income. Moreover, employing the available adults' birth weight data, we examine the channel of high-temperature effects on adult outcomes. Additionally, the heterogeneous effects of high temperatures across trimesters on all outcomes are outlined.

1.4.1 Effects on adult outcomes

We begin our analysis by presenting the effect of ambient heat during pregnancy on adult outcomes, which are shown in Table 1.1 to 1.4.[23]

Educational Attainment. Like the subsequent tables in this subsection, Table 1.1

[23]Individuals who did not survive due to exposure to ambient heat in the prenatal period are not included in our sample. We may underestimate welfare losses because of such selection (Black et al. 2007).

presents four specifications, indicating the impacts of ambient heat on educational achievements. In column (1), we include other weather controls and the county, birth year, and birth month fixed effects. Column (1) shows that the effect of high temperatures during pregnancy on educational attainment is negative and significant at the 10% level. By adding individual characteristics, Column (2) shows that one additional high-temperature day lowers the average education years by about 0.006 years. And the effect is significant at the 5% level. Interestingly, although the R-squared increases and the standard error declines, the point estimate on high-temperature days does not change much. This finding suggests that the high-temperature days during pregnancy in the specification are exogenous, consistent with what we find in Table A1.8.

Column (3) further includes the county-specific linear and quadratic trends, partialling out time-varying characteristics associated with both dependent and independent variables and are trending linearly and quadratically during the analysis period. The coefficient remains stable and statistically significant. Finally, in column (4) we replace the county-specific trends with the province-year fixed effects, which nonparametrically capture the nonlinear changes in the determinants of human capital formation. At this more stringent specification, the coefficient shows that a one standard deviation increase in high-temperature days (34.49 days) lowers the average years of schooling by 0.29(=0.0085*34.49) years (7.18% standard deviation). It indicates that without province-year fixed effects, the adverse effects would be underestimated. Moreover, precipitation does not have significant effect on educational attainment. Cold weather in pregnancy somehow have a positive influence on education years.[24] In addition, males, individuals with higher educational achievements parents, and younger children in one family tend to have longer years of schooling. The number of siblings (family size) is negatively correlated with the educational attainment, consistent with the findings in Black et al. (2005).

We have thus far defined the "high temperature" as a daily maximum temperature higher than 85°F. We acknowledge that this threshold is arbitrary to some degree. To test the sensitivity of the estimates to the temperature threshold, we apply different thresholds, ranging from 60°F to 90°F. Panel (a) in Figure 1.1 summarizes the coefficients and 90% confidence intervals for estimates of the effects on schooling years using thresholds from 60°F to 90°F, respectively. As can be seen, the coefficients for high-temperature days are significantly negative when the threshold is beyond 75°F, implying that the effects of high temperatures during pregnancy are not sensitive to the temperature thresholds.

Cognitive Abilities. Next, we examine the adverse effects of hot weather on adult cognitive abilities measured by standardized word- and math-test scores. As shown in Table 1.2, columns (1) through (4) replicate the specifications from Table 1.1. Without any background information controls, column (1) reveals that hot weather in the prenatal period has statistically significant adverse effect on word-test score. Compared to that in column (1), the richer specifications in columns (2) through (4) show that the absolute values of point estimates increase slightly. As the last column shows, adults who experienced one standard deviation more high-temperature days in the prenatal period

[24] The positive effect of cold weather during pregnancy is similar to that in Isen et al. (2015).

are 7.59% standard deviations lower for word-test score. And the effect is significant at the 5% level. What's more, the effects of low temperatures and total rainfall are not statistically different from zero. Furthermore, females, individuals with lower educational achievements parents, and elder children in one family perform worse in the word test.

Table 1.3 statistically summarizes the effect of high temperatures during pregnancy on the math-test score. Column (1) reports that adults who were exposed to one additional high-temperature day *in utero* stage are 0.09% standard deviations lower for math-test score, but this estimate is not statistically significant. In column (4), the richer specification, the estimate becomes statistically significant at the 10% level. We find that a one standard deviation increase in high-temperature days during the prenatal period lowers the math-test score by 4.83% standard deviations. Moreover, the effect of cold weather during pregnancy is again positive. Total rainfall during pregnancy does not significantly influence the math-test performance. Additionally, males, individuals with higher educational achievements parents, and younger children in one family tend to have higher math-test scores.

Panels (b) and (c) in Figure 1.1 display the sensitivity tests of temperature thresholds. For the word-test score, the negative effects of high temperatures are statistically significant across different thresholds. But the effect on math-test performance becomes statistically significant at the 10% level only when the threshold is beyond 80°F.

The temperature on the survey day may affect respondents' test performance (Zivin et al. 2015). And if it was also correlated with the high-temperature days during pregnancy, it may cause some bias for the results about word-test and math-test. Therefore, as a robustness check, we include county specific survey month fixed effects to partly control for weather situation during survey[25]. The coefficients show in Table A1.9 are reasonably robust. And the results are still statistical significant for word-test.

Height. Table 1.4 displays four specifications the same as preceding tables, indicating the impacts of ambient heat on height. Column (1) implies that hot weather during pregnancy has negative (but insignificant) effect on height. By adding personal background controls and county-specific trends, the estimates become more precise. Replacing the county-specific trend with province-year fixed effects in column (4), the absolute value of the high-temperature-days coefficient increases slightly. The point estimate shows that a one standard deviation increase in high-temperature days lowers the average height by 0.40 cm (5.16% standard deviation). Moreover, panel (d) in Figure 1.1 shows that the negative effect of high temperatures on height is not sensitive to the temperature threshold. Additionally, we find that cold weather does not have a significant effect, while the rainfall during prenatal period seems to have a marginally significant negative effect on height. Furthermore, males, Han Chinese, and individuals with higher educational achievements parents tend to be taller than others on average.

Labor Market Outcome. Table A1.10 assesses the impact of hot weather on personal annual income.[26] As shown in the tables above, individuals who were exposed to high

[25]We only have survey month information without knowing the exact survey date

[26]As we have discussed in the data section, the personal income variable is not measured precisely. Here the results are only suggestive evidence.

temperatures during the prenatal period attain fewer education years and have worsen cognitive abilities, which are crucial determinants for labor market outcomes. In the analysis sample, about 76% (8077 out of 10685) individuals report their annual income in 2010. In columns (1) through (4), the sign of the high-temperature-days coefficients is consistently negative. Although the coefficients are not statistically significant at the traditional level, the adverse effect is economically significant. Column (4), the more stringent specification, shows that a one standard deviation increase in high-temperature days during pregnancy lowers the annual income by 4.48%.What's more, other weather controls do not have significant effects on the labor market performance. Males and individuals with higher educational achievements parents tend to earn more annual income. Additionally, we find that the younger the individuals in one family, the more they earn. This pattern is consistent with those in previous tables, i.e., younger children in one family have more educational attainment and better cognitive abilities. Family size also negatively affects the labor market performance.

As we discussed in the model part, families may non-randomly sort across geographic regions within a province, for which case the province-year fixed effects are unable to capture. The fact that during our sample period, people in rural China are severely restricted on migration and relocation, does address the concern to some extent. We further replace the province-year fixed effects with county-year fixed effects to check the sensitivity of our estimates. As Table A1.11 shows, after controlling county-year fixed effects, the coefficients of high-temperature days during pregnancy are consistently negative for all adult outcomes. The results are relatively robust. Compared to the estimates with province-year fixed effects, those in Table A1.11 increase slightly. But as the models are more demanding of the data, the estimates are less precise.

We examine the effects of high temperatures during the gestational period for urban-born individuals as well. We find that high-temperature days have no effect on adult outcomes for urban individuals, either statistically or economically (even no systematic direction of the impacts; see Table A1.12 for related results). This is possibly because living conditions—e.g., housing quality and the availability of cooling tools—in urban areas are much better than those in rural areas. Also, urban individuals typically work outside less frequently, and thus are less likely to be directly exposed to ambient heat. However, based on the time use data, Peterman et al. (2013) find that in rural China, the time spent on agricultural work for the women during pregnancy is not statistically different from that for non-pregnant women. It implies that pregnant women in rural China are exposed to ambient heat more frequently and directly. Therefore, we will focus on the rural sample from this point on.

1.4.2 Effect on birth weight

In this subsection, we examine the effect of ambient heat during pregnancy on birth weight—a possible important channel explaining high-temperature effects on adult outcomes.[27] The adverse effect of ambient heat during pregnancy on birth weight is present-

[27] We acknowledge that birth weight may not be the only channel through which high temperatures during pregnancy affect adult outcomes. Therefore, IV estimates may not satisfy the exclusion restriction

ed in Table 1.5. Columns (1) through (4) repeat the specifications from Table 1.1 and reveal large and precisely estimated effect of hot weather in the prenatal period on birth weight. In particular, the richest specification in column (4) shows that birth weight is 2.14 grams (around 0.07%) lower for one additional high-temperature day. And the effect is statistically significant at the 5% level. To compare, Deschenes et al. (2009) find that each additional day>85°F lowers birth weight by 0.0025%. The magnitude is much smaller than our estimate, which is possibly because living conditions in the US were much better than those in China during the sample period. Such adverse influence is not negligible. A one standard deviation increase in high-temperature days leads to a 73.86 grams drop in birth weight, which is about 12.35% standard deviation. Moreover, cold weather does not have significant effect on birth weight, whereas high rainfall affects it negatively. Males and individuals with higher educational achievements parents and who live in small families have higher birth weight.

Table 1.6 presents for effects of high temperatures on LBW incidence. Without any family background controls, column (1) implies that high temperatures in the prenatal period has statistically significantly increase the risk for LBW. Relative to that in column (1), the more stringent specifications in columns (2) through (4) show that the point estimates remain stable. Column (4) indicates that one extra hot-weather day significantly increases the risk for LBW by 0.12 percentage points. In addition, the effect of low temperatures is not statistically different from zero, but precipitation increases the risk. For family back-

assumption.

ground controls, gender and father's education achievements significantly affect the risk for LBW.

As mentioned above, about one-third of individuals in the full sample remembered their own birth weight. And birth weight is less likely to be missing for individuals from better-educated families. This suggests that our estimated effects of high temperatures during pregnancy may be biased downwards since the effects are likely to be larger for individuals with worse family background. In Table A1.2, we examine whether the impacts of hot weather are smaller for individuals with non-missing birth weight data. As row (1) indicates, the interaction term between high-temperature days and birth-weight-non-missing indicator is not statistically significant for any adult outcomes. This finding implies that birth weight does not appear to be missing in a systematic way that would influence our results.

We also run sensitivity checks by using different definitions of high-temperature days. Panels (e) and (f) in Figure 1.1 show the coefficients and 90% confidence intervals for estimates of birth weight and low-birth-weight incidence using thresholds from 60°F to 90°F, respectively. As can be seen, the effects of high temperatures during pregnancy are not sensitive to the temperature thresholds.

1.4.3 Trimester heterogeneity

In subsequent analyses in this section, we allow for heterogeneous effects of ambient heat across trimesters. Table 1.7 illustrates the effects of high temperatures in each trimester. In each regression, we include all personal background controls, birth-month fixed effects, and province-year two-way fixed effects. Columns (1) through (4) present the results for adult outcomes. Noticeably, the adverse effects of hot weather are more significant and larger in the second trimester for all outcomes.[28] Specifically, individuals who were exposed to one standard deviation more high-temperature days in the second trimester (around 21.86 days) attain 0.30 fewer education years, are 7.21% and 5.68% standard deviations lower for word- and math-test scores, and are 0.36 cm shorter, respectively. However, high temperatures in other trimesters do not have significant effects on these adult outcomes.

In addition, we observe similar patterns for birth weight. Column (5) shows that high-temperature days in the second trimester significantly lower birth weight. A one standard deviation increase in the number of high-temperature days in the second trimester leads to a loss of 68.83 grams of birth weight (12.14% standard deviation). Moreover, column (6) shows that one additional hot-weather day increases the probability of low-birth-weight incidence by 0.14 percentage points in the second trimester. Such sensitivity to temperature fluctuation during the second trimester has also been documented by medical research (Murray et al. 2000; Elter et al. 2004). However, as we do not have precise data on birth date or gestational length, trimesters are defined with errors. Therefore, we cautiously conclude that pregnant women are more sensitive to ambient heat in the second trimester.

[28] Wald tests show that the coefficients of the second trimester are statistically significantly different from those in the other two trimesters. Additionally, the coefficients in the first trimester are not statistically significantly different from those in the third trimester.

1.5 Discussion

1.5.1 Mechanisms

Our results thus far have presented the effects of high temperatures during pregnancy on adult outcomes and birth weight. Several channels may account for such impacts. One possibility is that hot weather has adverse physiological influences on pregnant women due to physical and mental strain.[29] By affecting the pregnant woman's health, heat stress further triggers negative impacts on newborns—e.g., low birth weight. In addition to physiological effects, high temperatures may also cause damage to crop yields (Hollinger and Angel 2009; Schlenker and Roberts 2009; Burgess et al. 2011), which determine family resources in rural areas and influence newborns' endowment through income effects, as suggested by Maccini and Yang (2009). Furthermore, hot weather may induce changes in daily activities (behavioral effects). For instance, Herman (1993) documents that ambient heat could reduce appetite and alter food selection. Food intake and selection are critical for fetal development.

To test the income effects, we exploit the variation of the crops' sensitivity to heat. Specifically, C4 plants, including corn, sugarcane, and sorghum, are more adaptable to hot weather due to the efficient way they retain water in a hot environment. In contrast, C3 plants (barley, rice, wheat, etc.) are more sensitive to heat stress. If income effects

[29] Strand et al. (2011) suggest that a pregnant woman may be sensitive to heat stress because (i) the capacity to lose heat by sweating is lessened due to the reduced ratio of surface area to body mass, (ii) weight gain triggers more heat production, (iii) core temperature increases with accumulated fat deposition, and (iv) the increased body composition and metabolic rate of the fetus cause a rise in maternal heat stress.

matter, people living where C4 (C3) plants are widely cultivated would be less (more) affected by high temperatures during pregnancy. To test the income channel, we employ the following specification:

$$Y_{ijmt} = \beta HighTemp_{ijmt} + \zeta C4PlantArea_{pt} + \kappa HighTemp_{ijmt} * C4PlantArea_{pt} + W^{'}_{ijmt}\gamma + X^{'}_{i}\delta + \mu_j + \lambda_t + \eta_m + \theta_{pt} + \epsilon_{ijmt}. \quad (1.3)$$

Here, p references province. $HighTemp_{ijmt}$ denotes number of days with a daily maximum temperature higher than 85°F during pregnancy. $C4PlantArea_{pt}$ represents the corn and sugarcane proportion of crop acreage within each province-year cell.[30] The other notations are the same as those in Equation (1). If high temperatures affect people through the income channel, we would expect the coefficient of interaction term κ to be significantly positive. As shown in Table A1.13, the interaction terms are not statistically significant for any outcomes; neither do they have a consistent direction of impacts. Also, the coefficients for high-temperature days change slightly. The results provide no support for the existence of income effects before birth, consistent with the findings of Maccini and Yang (2009).

Furthermore, in the main results section, we find that the negative effects of high temperatures during pregnancy are concentrated in the second trimester, while they are not

[30] County-level plant area data are not available before 1997. Instead, we use plant-area data from the Thematic Database for Human-earth System (http://www.data.ac.cn/zrzy/DH55.asp?name=&pass=&danwei=). It provides the plant area of each crop within each province from 1949 to 2000. There are two C4 crops (corn and sugarcane) in the dataset; the other 8 crops are C3 plants.

economically and statistically significant in other trimesters (see Table 1.7). This pattern can be explained by income effects only if there are large overlaps between growing seasons and the second trimester. However, birth months in our sample are not concentrated in any certain seasons, and the high-temperature days across trimesters are similar to each other (see Table A1.5). Based on the three pieces of evidence, we conclude that the adverse effects of high temperatures during pregnancy are less likely to be triggered by income effects.

Finally, we acknowledge that distinguishing the physiological and behavioral effects is challenging as the data do not contain information on individual activities during pregnancy. But it is hard to imagine that pregnant women change food intake and selection only at the second trimester as a result of ambient heat. As we do not have any direct evidences, we cannot fully pin down the mechanisms of the adverse effects of high temperatures during pregnancy. But the results above conservatively suggest that the adverse effects may be caused by physiological effects.

1.5.2 Nonlinear effects of hot weather

In this subsection, we explore the nonlinear effects of high temperatures on adult outcomes and birth weight. If ambient heat adversely affected embryos (or fetuses) only beyond a certain level of accumulated high-temperature days, it would change welfare implications, since high frequency of high-temperature days is not that common. We employ the partially linear model, allowing the key variable to be nonlinear:[31]

$$Y_{ijmt} = f(X_{ijmt}) + Z^{'}\gamma + \epsilon_{ijmt}. \tag{1.4}$$

where X_{ijmt} represents the number of hot-weather days during pregnancy. $f(.)$ is the unspecified nonlinear component, estimated by kernel regression with optimal bandwidth.[32] Z represents other controls and fixed effects in Equation (1). To estimate Equation (4), we use the Robinson difference estimator (Robinson 1988).

Panels (a) through (d) of Figure A1.5 present the adult welfare outcomes estimates from Equation (4).[33] The y-axis represents the dependent variable partialled out from the parametric fit. The relationships shown in the figure are striking (significant at the 1% level): When the number of high-temperature days during pregnancy increases, years of schooling, word- and math-test scores, and height decrease monotonically.

The effect of high temperatures during pregnancy on birth weight is presented in Panel (e) of Figure A1.5. The non-parametric relationship is significant at the 1% level. The adverse effect on birth weight seems to decline almost linearly when there are fewer than 100 high-temperature days; Again, estimate beyond 100 high-temperature days is not precise.

[31]The partially linear model was first applied by Engle et al. (1986).

[32]The Epanechnikov kernel function is applied here.

[33]As Robinson (1988) points out, the nonparametric estimators of $E(Y|X_{ijmt})$ and $E(Z|X_{ijmt})$ are not reliable if the density of X_{ijmt} is close to zero at x. Therefore, all regressions in Figure A1.5 are performed on a trimmed sample, in which 3% observations with the lowest estimated density are excluded.

1.5.3 Falsification tests and effects of high temperatures after birth

In this subsection, we first provide a falsification test by examining the impacts of high temperatures after birth on birth weight. In principle, birth weight should not be influenced by after-birth temperatures. As shown in columns (1) and (2) in Table A1.14, the effects of high temperatures during nine months after birth are not statistically significant at the traditional level.[34] Additionally, low temperatures and precipitation do not affect birth weight as well. In addition to after-birth weather conditions, temperatures and precipitation before conception are supposed to not influence birth weight and later outcomes as well. Table A1.15 shows that all adult outcomes are not affected by high temperatures during six months before conception[35]. Additionally, the coefficient of high-temperature days in columns (5) is positive and far from statistically significant, indicating no effect on birth weight. The two falsification tests provide support to our main specification.

Besides the *in utero* stage, the early life conditions after birth are critical for human capital development as well (Almond et al. 2009). In regressions, we replace high-temperature days during pregnancy in Equation (1) with those during nine months after birth. As displayed in columns from (3) through (6) in Table A1.14, the number of high-temperature days during nine months after birth is negatively associated with all adult outcomes. Compared to the effects of high temperatures during pregnancy, the negative effects during nine months after birth are smaller and less significant, except for height.

[34]The results are very similar by examining three months and six months after birth.

[35]Six months before conception is defined as ten to fifteen months before conception. Such results are also robust to three months and nine months before the conception.

1.5.4 Predicting the impacts of climate change on birth weight and adult outcomes

The effects of high temperatures during pregnancy have not been taken into account when calculating the costs of global warming. Based on climate projections provided by the National Aeronautics and Space Administration (NASA), we perform back-of-the-envelope predictions for birth and adult outcomes of individuals born in rural areas of China in 2100. Our predictions strongly rely on the assumption that all other related factors will remain constant. We acknowledge that individuals can adapt to changing environment and mitigate the potential negative effects of increasing temperatures. Moreover, access to air conditioners have been drastically increasing in China and other developing countries in recent years. But back-of-the-envelope predictions here can provide us a concrete figure, indicating without the technology progress and adaption what the specific effects of global warming are.

Assuming that greenhouse gas emissions will peak around 2040 (RCP 4.5 scenario), we predict that, holding all else equal, babies born in rural areas of China in 2100, on average, will weigh 10 grams less than those born in 2000 due to global warming.[36] Further, those individuals in adulthood will suffer 0.10 fewer years of schooling and 0.14 cm decrease in height.[37] In an even more pessimistic case (RCP 8.5 scenario), greenhouse

[36]RCPs are possible greenhouse-gas-concentration trajectories adopted by the Intergovernmental Panel on Climate Change (IPCC). Specifically, RCP 4.5 presumes that global annual greenhouse gas emissions (measured in CO2-equivalents) will peak around 2040, then decrease. In RCP 8.5, emissions keep increasing throughout the 21st century. Under the RCP 4.5 scenario, the number of high-temperature days during pregnancy increases by 12.35 days on average.

[37]As cold weather in the analysis does not have significant effects on most outcomes, it is not taken into

gas emissions will peak around 2100 and birth weight loss will rise sharply to 50 grams.[38] Likewise, losses in education years and height will increase to 0.40 years and 0.55 cm, respectively. Again, the above predictions are based on a strong assumption that all other related factors will remain constant—i.e., the same purchasing power, medical technologies, and access to air conditioners (Barreca et al. 2016 and Zivin et al. 2015). As other factors are being improved in China, however—especially in rural areas—the effects of global warming may be alleviated.

1.6 Conclusions

In this paper, we examine the long-term effects of high temperatures during the prenatal period on education attainment, cognitive abilities, physical conditions, and labor market outcome. Additionally, we investigate whether prenatally exposed children have worse endowments—birth weight. Our results indicate that hot weather in early life not only trigger adverse birth outcomes, but have persistent and profound effects in later life. By enduring one additional standard deviation of hot-weather days *in utero* (about 34 days), individuals attain 0.29 fewer years of schooling, are 7.59% and 4.83% standard deviations lower for word- and math-test scores, and grow to be 0.40 cm shorter. The impacts seem to be concentrated in the second trimester. Moreover, children who were prenatally exposed to frequent heat stress have statistically significant lower birth weight and higher risk

account in this back-of-the-envelope predictions.

[38] Under the RCP 8.5 scenario, the number of high-temperature days during pregnancy increases by 47.87 days on average.

for LBW. We also examine the possible mechanisms behind the adverse effects of hot weather during pregnancy. Several pieces of evidence suggest that the adverse effects of high temperatures during pregnancy are less likely to be triggered by income effects.

Tables and Figures of Chapter One

Table 1.1: The impact of high temperatures during pregnancy on educational attainment

	(1)	(2)	(3)	(4)
Dependent Variable:	Eduy	Eduy	Eduy	Eduy
High Temp Days	-0.0053*	-0.0059**	-0.0058**	-0.0085**
	(0.0030)	(0.0027)	(0.0026)	(0.0035)
Low Temp Days	0.0036	0.0043*	0.0040*	0.0052**
	(0.0024)	(0.0023)	(0.0023)	(0.0024)
Precipitation (log)	0.0898	0.0621	0.1004*	0.0733
	(0.0579)	(0.0566)	(0.0524)	(0.0703)
Female		-1.3052***	-1.3390***	-1.3376***
		(0.1141)	(0.1166)	(0.1206)
Han		0.1688	0.1703	0.0945
		(0.4071)	(0.4156)	(0.4323)
Mother's Education Years		0.1085***	0.1217***	0.1096***
		(0.0109)	(0.0110)	(0.0112)
Mother's Age at Birth		0.0308***	0.0320***	0.0346***
		(0.0107)	(0.0104)	(0.0109)
Father's Education Years		0.1587***	0.1556***	0.1562***
		(0.0116)	(0.0121)	(0.0118)
Father's Age at Birth		-0.0206**	-0.0206**	-0.0198**
		(0.0098)	(0.0097)	(0.0095)
Birth Order		0.0282	0.0328	0.0284
		(0.0391)	(0.0398)	(0.0436)
Number of Siblings		-0.0782**	-0.0627*	-0.0516
		(0.0334)	(0.0357)	(0.0374)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	10685	10685	10685	10685
R-Squared	0.269	0.332	0.370	0.404

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 1.2: The impact of high temperatures during pregnancy on word-test score

	(1)	(2)	(3)	(4)
Dependent Variable:	Word Test	Word Test	Word Test	Word Test
High Temp Days	-0.0016**	-0.0017**	-0.0015**	-0.0022**
	(0.0007)	(0.0007)	(0.0007)	(0.0009)
Low Temp Days	0.0006	0.0007	0.0005	0.0008
	(0.0006)	(0.0005)	(0.0006)	(0.0006)
Precipitation (log)	0.0005	-0.0045	0.0063	-0.0028
	(0.0115)	(0.0109)	(0.0128)	(0.0151)
Female		-0.2902***	-0.2965***	-0.2948***
		(0.0302)	(0.0317)	(0.0328)
Han		0.1119	0.1081	0.0842
		(0.1155)	(0.1171)	(0.1199)
Mother's Education Years		0.0183***	0.0204***	0.0190***
		(0.0025)	(0.0026)	(0.0027)
Mother's Age at Birth		0.0003	0.0012	0.0019
		(0.0027)	(0.0027)	(0.0029)
Father's Education Years		0.0323***	0.0319***	0.0310***
		(0.0029)	(0.0029)	(0.0031)
Father's Age at Birth		0.0007	0.0003	-0.0008
		(0.0022)	(0.0022)	(0.0022)
Birth Order		0.0013	-0.0018	0.0035
		(0.0099)	(0.0100)	(0.0106)
Number of Siblings		-0.0020	0.0044	0.0058
		(0.0092)	(0.0093)	(0.0102)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	10685	10685	10685	10685
R-Squared	0.306	0.348	0.382	0.416

Notes: An observation is an individual born in a rural area. For convenience of interpretation, word-test score is standardized. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 1.3: The impact of high temperatures during pregnancy on math-test score

	(1)	(2)	(3)	(4)
Dependent Variable:	Math Test	Math Test	Math Test	Math Test
High Temp Days	-0.0009	-0.0011	-0.0008	-0.0014*
	(0.0008)	(0.0007)	(0.0007)	(0.0009)
Low Temp Days	0.0007	0.0009	0.0006	0.0011*
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Precipitation (log)	0.0212*	0.0145	0.0236*	0.0047
	(0.0120)	(0.0114)	(0.0137)	(0.0145)
Female		-0.3273***	-0.3317***	-0.3291***
		(0.0307)	(0.0319)	(0.0330)
Han		0.0587	0.0618	0.0375
		(0.1157)	(0.1169)	(0.1187)
Mother's Education Years		0.0264***	0.0290***	0.0266***
		(0.0027)	(0.0028)	(0.0030)
Mother's Age at Birth		0.0068***	0.0075***	0.0080***
		(0.0024)	(0.0025)	(0.0026)
Father's Education Years		0.0387***	0.0380***	0.0376***
		(0.0031)	(0.0032)	(0.0032)
Father's Age at Birth		-0.0041*	-0.0043*	-0.0047*
		(0.0024)	(0.0024)	(0.0024)
Birth Order		0.0023	-0.0004	0.0022
		(0.0094)	(0.0092)	(0.0102)
Number of Siblings		-0.0102	-0.0040	-0.0019
		(0.0089)	(0.0087)	(0.0094)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	10685	10685	10685	10685
R-Squared	0.267	0.328	0.365	0.396

Notes: An observation is an individual born in a rural area. For convenience of interpretation, math-test score is standardized. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 1.4: The impact of high temperatures during pregnancy on height

	(1)	(2)	(3)	(4)
Dependent Variable:	Height	Height	Height	Height
High Temp Days	-0.0050	-0.0083**	-0.0100**	-0.0115**
	(0.0054)	(0.0039)	(0.0039)	(0.0048)
Low Temp Days	-0.0043	0.0039	0.0057	0.0043
	(0.0052)	(0.0036)	(0.0038)	(0.0042)
Precipitation (log)	-0.1082	-0.1706**	-0.1807**	-0.1883*
	(0.1088)	(0.0822)	(0.0856)	(0.0993)
Female		-10.1393***	-10.1589***	-10.1642***
		(0.1309)	(0.1339)	(0.1372)
Han		0.8951***	0.9618***	0.8575**
		(0.3404)	(0.3421)	(0.3393)
Mother's Education Years		0.0701***	0.0673***	0.0654***
		(0.0197)	(0.0200)	(0.0204)
Mother's Age at Birth		0.0227	0.0181	0.0214
		(0.0177)	(0.0176)	(0.0176)
Father's Education Years		0.0364**	0.0320*	0.0333*
		(0.0172)	(0.0176)	(0.0185)
Father's Age at Birth		-0.0189	-0.0138	-0.0183
		(0.0166)	(0.0167)	(0.0168)
Birth Order		0.0546	0.0595	0.0457
		(0.0704)	(0.0738)	(0.0734)
Number of Siblings		-0.0877	-0.0869	-0.0677
		(0.0602)	(0.0631)	(0.0606)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	10685	10685	10685	10685
R-Squared	0.105	0.532	0.550	0.576

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 1.5: The impact of high temperatures during pregnancy on birth weight

	(1)	(2)	(3)	(4)
Dependent Variable:	Birth Weight	Birth Weight	Birth Weight	Birth Weight
High Temp Days	-1.8112**	-1.7947**	-1.8779**	-2.1416**
	(0.7474)	(0.7578)	(0.8265)	(1.0533)
Low Temp Days	-0.2537	-0.0364	-0.4772	-0.4953
	(0.6882)	(0.6898)	(0.7285)	(0.8746)
Precipitation (log)	-22.1543	-23.2652	-52.5685*	-64.7445*
	(23.9229)	(22.3179)	(27.8025)	(33.2134)
Female		-139.9982***	-127.2844***	-144.6298***
		(19.5494)	(21.3040)	(23.6459)
Han		42.9536	47.0244	37.6086
		(84.7643)	(91.0640)	(106.7231)
Mother's Education Years		5.3377*	2.7866	1.8764
		(2.9393)	(3.1656)	(3.7037)
Mother's Age at Birth		-4.2066	-2.5088	-5.0695
		(3.0642)	(3.1336)	(3.1866)
Father's Education Years		5.8275**	6.5777**	6.4526*
		(2.9389)	(3.0958)	(3.5854)
Father's Age at Birth		-0.2452	-0.4145	0.6383
		(2.7278)	(2.8870)	(3.2229)
Birth Order		30.0528**	24.7402*	19.4080
		(12.6483)	(14.0023)	(15.9998)
Number of Siblings		-25.0534**	-22.8363**	-23.4000*
		(9.6602)	(10.9776)	(12.8334)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	3223	3223	3223	3223
R-Squared	0.224	0.244	0.324	0.414

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 1.6: The impact of high temperatures during pregnancy on risk for LBW

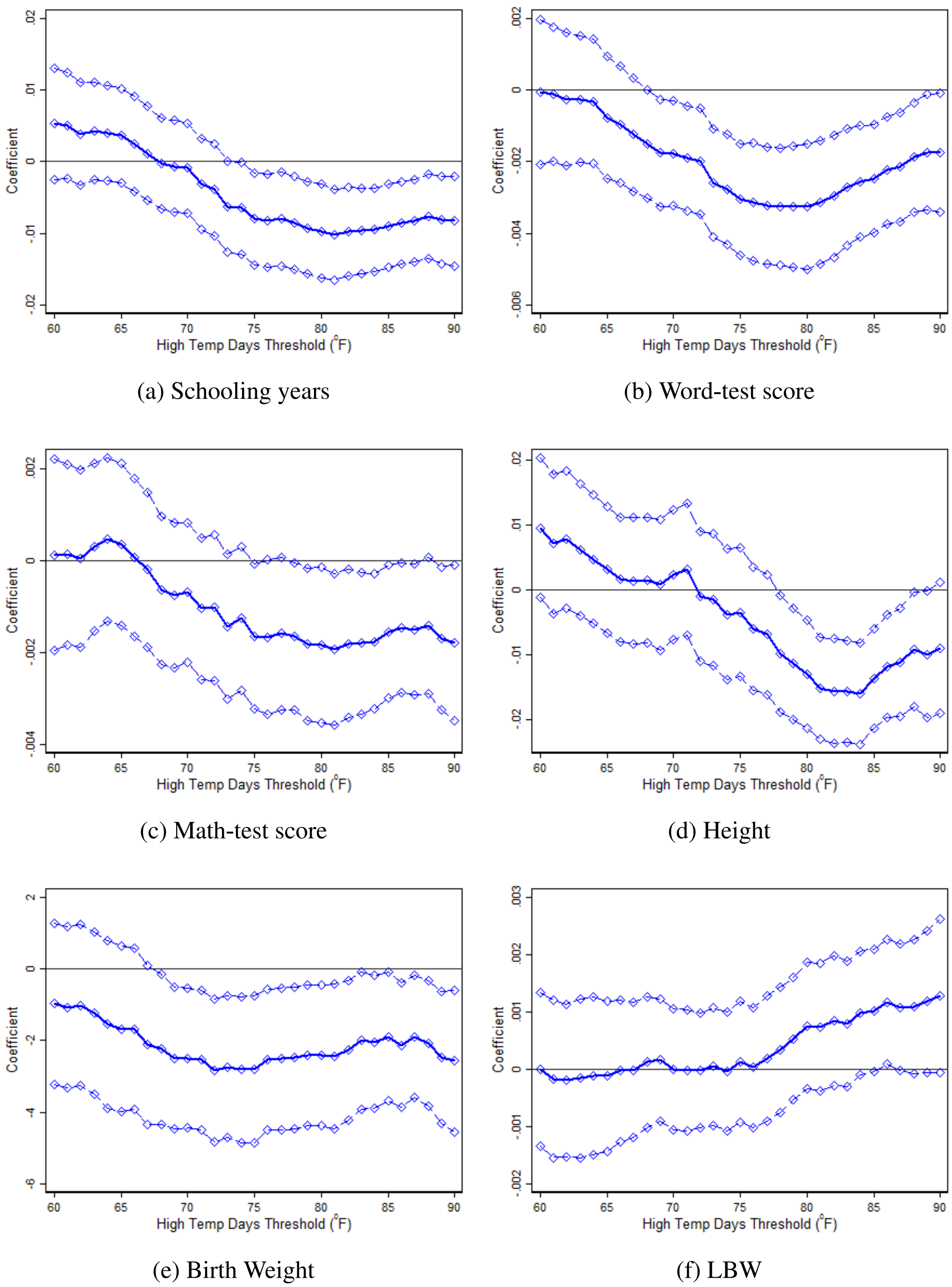
	(1)	(2)	(3)	(4)
Dependent Variable:	LBW	LBW	LBW	LBW
High Temp Days	0.0010**	0.0009**	0.0010*	0.0012*
	(0.0005)	(0.0005)	(0.0005)	(0.0007)
Low Temp Days	-0.0001	-0.0002	0.0001	0.0001
	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Precipitation (log)	0.0093	0.0106	0.0149	0.0320*
	(0.0131)	(0.0131)	(0.0151)	(0.0192)
Female		0.0126	0.0062	0.0237*
		(0.0107)	(0.0104)	(0.0141)
Han		0.0069	0.0090	0.0119
		(0.0214)	(0.0251)	(0.0296)
Mother's Education Years		-0.0028*	-0.0018	-0.0016
		(0.0015)	(0.0017)	(0.0019)
Mother's Age at Birth		0.0013	0.0012	0.0022
		(0.0017)	(0.0019)	(0.0020)
Father's Education Years		-0.0028*	-0.0036**	-0.0041**
		(0.0015)	(0.0016)	(0.0018)
Father's Age at Birth		-0.0002	-0.0005	-0.0009
		(0.0014)	(0.0016)	(0.0019)
Birth Order		-0.0046	-0.0026	-0.0007
		(0.0078)	(0.0085)	(0.0103)
Number of Siblings		0.0015	0.0002	0.0025
		(0.0045)	(0.0057)	(0.0061)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	3223	3223	3223	3223
R-Squared	0.147	0.150	0.236	0.322

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table 1.7: The impacts of high temperatures during pregnancy on adult outcomes and birth weight by trimester

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days (1st trimester)	-0.0011	-0.0008	0.0002	-0.0057	-0.7833	0.0007
	(0.0047)	(0.0011)	(0.0012)	(0.0070)	(1.5058)	(0.0008)
High Temp Days (2nd trimester)	-0.0137***	-0.0033***	-0.0026***	-0.0165***	-3.1487***	0.0014*
	(0.0045)	(0.0011)	(0.0010)	(0.0062)	(1.3787)	(0.0008)
High Temp Days (3rd trimester)	-0.0051	-0.0011	-0.0004	0.0011	-0.5627	0.0003
	(0.0058)	(0.0014)	(0.0014)	(0.0075)	(1.8109)	(0.0010)
Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685	3223	3223
R-Squared	0.404	0.417	0.397	0.577	0.415	0.324

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Each trimester consists of three months. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.



(a) Schooling years

(b) Word-test score

(c) Math-test score

(d) Height

(e) Birth Weight

(f) LBW

Figure 1.1: Coefficients of high-temperature days on adult and birth outcomes from regressions using different definitions of high-temperature days.

Notes: The solid line denotes the estimated coefficients on each high-temperature day definition. Dash lines represent the upper and lower bounds for the 90% confidence interval.

Appendix of Chapter One

Table A1.1: The effects of high temperatures during the eight months before birth on all outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days (8 Mon)	-0.0091**	-0.0021**	-0.0016*	-0.0086*	-1.6429*	0.0009
	(0.0035)	(0.0009)	(0.0009)	(0.0049)	(0.9579)	(0.0006)
Low Temp Days (8 Mon)	0.0053**	0.0009	0.0011*	0.0041	0.0130	-0.0001
	(0.0023)	(0.0006)	(0.0006)	(0.0039)	(0.7249)	(0.0004)
Precipitation (8 Mon)	-0.0002	-0.0000	-0.0000	-0.0001	-0.1160*	0.0000
	(0.0002)	(0.0001)	(0.0000)	(0.0004)	(0.0677)	(0.0000)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685	3223	3223
R-Squared	0.404	0.416	0.396	0.576	0.413	0.321

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F during the eight months before birth. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.2: Robustness checks of effects of high temperatures during pregnancy for the two group individuals—with and without birth weight data.

	(1)	(2)	(3)	(4)
Dependent Variable:	Eduy	Word Test	Math Test	Height
High Temp Days X Birth Weight Missing	-0.0032	-0.0007	-0.0001	0.0027
	(0.0029)	(0.0007)	(0.0007)	(0.0044)
High Temp Days	-0.0062	-0.0017*	-0.0014	-0.0135**
	(0.0042)	(0.0010)	(0.0009)	(0.0061)
Birth Weight Missing	-0.0409	0.0166	-0.0533	-0.2377
	(0.1750)	(0.0397)	(0.0428)	(0.2685)
Low Temp Days	0.0051**	0.0008	0.0011*	0.0045
	(0.0025)	(0.0007)	(0.0006)	(0.0043)
Precipitation (log)	0.0704	-0.0032	0.0038	-0.1897*
	(0.0708)	(0.0153)	(0.0144)	(0.0993)
Female	-1.3337***	-0.2980***	-0.3232***	-10.1620***
	(0.1201)	(0.0332)	(0.0325)	(0.1375)
Han	0.1025	0.0861	0.0390	0.8601**
	(0.4325)	(0.1213)	(0.1172)	(0.3394)
Mother's Education Years	0.1088***	0.0192***	0.0260***	0.0649***
	(0.0111)	(0.0027)	(0.0030)	(0.0204)
Mother's Age at Birth	0.0345***	0.0019	0.0078***	0.0213
	(0.0108)	(0.0029)	(0.0026)	(0.0177)
Father's Education Years	0.1558***	0.0314***	0.0369***	0.0327*
	(0.0119)	(0.0031)	(0.0032)	(0.0184)
Father's Age at Birth	-0.0197**	-0.0008	-0.0045*	-0.0181
	(0.0095)	(0.0022)	(0.0023)	(0.0169)
Birth Order	0.0281	0.0035	0.0020	0.0452
	(0.0437)	(0.0107)	(0.0101)	(0.0734)
Number of Siblings	-0.0499	0.0060	-0.0013	-0.0668
	(0.0374)	(0.0103)	(0.0093)	(0.0606)
County FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685
R-Squared	0.404	0.416	0.396	0.576

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F during the eight months before birth. Ordinary least squares estimates. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.3: Robustness checks of the impacts of high temperatures on all outcomes using weather stations within 60 km radius

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0068*	-0.0014	-0.0005	-0.0105**	-2.8805***	0.0015**
	(0.0037)	(0.0010)	(0.0009)	(0.0048)	(1.0729)	(0.0007)
Low Temp Days	0.0038	0.0002	0.0004	0.0046	0.0613	0.0000
	(0.0027)	(0.0007)	(0.0007)	(0.0046)	(0.9020)	(0.0005)
Precipitation (log)	0.0150	-0.0043	-0.0056	-0.1290	-64.3984***	0.0404***
	(0.0689)	(0.0121)	(0.0149)	(0.1081)	(22.0220)	(0.0130)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9850	9850	9850	9850	2977	2977
R-Squared	0.399	0.397	0.389	0.582	0.423	0.346

Notes: An observation is an individual born in a rural area. Each county is matched to all weather stations within 60 km. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F during the eight months before birth. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.4: Robustness checks of the impacts of high temperatures on all outcomes using weather stations within 100 km radius

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0050	-0.0017*	-0.0006	-0.0122**	-1.8773*	0.0010
	(0.0036)	(0.0009)	(0.0008)	(0.0048)	(1.0650)	(0.0007)
Low Temp Days	0.0044*	0.0008	0.0009	0.0042	-0.6361	0.0001
	(0.0025)	(0.0007)	(0.0006)	(0.0042)	(0.8535)	(0.0004)
Precipitation (log)	0.1504**	0.0223	0.0280	-0.2713***	-71.4837**	0.0283
	(0.0731)	(0.0203)	(0.0170)	(0.0874)	(32.6139)	(0.0230)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10932	10932	10932	10932	3268	3268
R-Squared	0.404	0.421	0.398	0.574	0.410	0.321

Notes: An observation is an individual born in a rural area. Each county is matched to all weather stations within 100 km. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F during the eight months before birth. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.5: Summary statistics

Variable	Mean	Std. Dev.	N
Education Years	6.67	4.04	10685
Standardized Word-test Score	0	1	10685
Standardized Math-test Score	0	1	10685
Annual Income (2010 CNY)	9648.6	20412.61	9841
Height (cm)	164.39	7.68	10685
Birth Weight (gram)	2971.5	566.75	3223
Low Birth Weight Dummy (<2,500 grams)	0.09	0.29	3223
Age	36.27	11.57	10685
Female	0.48	0.5	10685
Han Chinese	0.9	0.29	10685
Mother's Education Years	2.27	3.52	10685
Mother's Age at Birth	27.16	6.12	10685
Father's Education Years	4.37	4.26	10685
Father's Age at Birth	29.67	6.91	10685
Birth Order	2.21	1.47	10685
Number of Siblings	2.73	1.79	10685
High Temp Days	48.31	34.49	10685
High Temp Days (1st trimester)	15.06	21.28	10685
High Temp Days (2nd trimester)	16.41	21.86	10685
High Temp Days (3rd trimester)	16.84	22.53	10685
Low Temp Days	60.49	51.33	10685
Low Temp Days (1st trimester)	20.87	29.47	10685
Low Temp Days (2nd trimester)	18.92	28.23	10685
Low Temp Days (3rd trimester)	20.7	29.64	10685
Precipitation (log)	6.27	0.92	10685
Total Precipitation(1st trimester, log)	4.79	1.31	10685
Total Precipitation(2nd trimester, log)	4.87	1.29	10685
Total Precipitation(3rd trimester, log)	4.81	1.34	10685

Notes: The sample contains 10,685 individuals in 143 counties across 25 provinces. All individuals in the sample were born in rural areas. High-temperature days are defined as those with a daily maximum temperature higher than 85°F. For convenience of interpretation, word- and math-test scores are standardized. In the sample, around 30% individuals did not report annual income, either because they did not work at the survey year or because they have already retired.

Table A1.6: The impacts of high temperatures on all outcomes (three two-way fixed effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0079	-0.0024*	-0.0022*	-0.0126*	-2.4396	0.0013
	(0.0050)	(0.0013)	(0.0012)	(0.0073)	(1.8310)	(0.0011)
Low Temp Days	0.0113	0.0020	0.0027	0.0169*	-0.3866	0.0004
	(0.0071)	(0.0020)	(0.0020)	(0.0090)	(2.1706)	(0.0015)
Precipitation (log)	0.1397*	0.0025	0.0167	-0.2104**	-89.0660**	0.0494**
	(0.0773)	(0.0167)	(0.0149)	(0.1058)	(39.0170)	(0.0223)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Province - Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province - Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year - Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685	3223	3223
R-Squared	0.452	0.460	0.445	0.611	0.589	0.511

Notes: An observation is an individual born in an rural area. High-temperature days are defined as those with daily maximum temperature higher than 85°F. Ordinary least squares estimates for all columns. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.7: The impacts of high temperatures on all outcomes clustered at the province level

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0085***	-0.0022***	-0.0014**	-0.0115***	-2.1416*	0.0012**
	(0.0027)	(0.0007)	(0.0007)	(0.0041)	(1.0401)	(0.0005)
Low Temp Days	0.0052**	0.0008	0.0011*	0.0043	-0.4953	0.0001
	(0.0023)	(0.0005)	(0.0006)	(0.0026)	(0.6331)	(0.0003)
Precipitation (log)	0.0733	-0.0028	0.0047	-0.1883*	-64.7445**	0.0320**
	(0.0540)	(0.0129)	(0.0128)	(0.1086)	(28.1537)	(0.0115)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685	3223	3223
R-Squared	0.404	0.416	0.396	0.576	0.414	0.322

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F during the eight months before birth. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates. Standard errors in parentheses, clustered by province. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.8: Balanced check on observable characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Female	Han Race	ME	MA	FE	FA	Birth Order	Num. of Siblings
High Temp Days	-0.0007	-0.0002	0.0024	0.0085	-0.0014	0.0081	0.0007	-0.0013
	(0.0005)	(0.0002)	(0.0031)	(0.0055)	(0.0037)	(0.0057)	(0.0012)	(0.0014)
Low Temp Days	0.0006*	0.0002	-0.0027	-0.0054	0.0035	-0.0069	-0.0006	-0.0001
	(0.0004)	(0.0002)	(0.0025)	(0.0043)	(0.0031)	(0.0048)	(0.0009)	(0.0011)
Precipitation (log)	-0.0071	-0.0008	0.0659	-0.0526	0.0298	0.0169	-0.0311	-0.0301
	(0.0072)	(0.0018)	(0.0610)	(0.1496)	(0.0662)	(0.1784)	(0.0382)	(0.0260)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685	10685	10685	10685	10685
R-Squared	0.107	0.649	0.324	0.166	0.294	0.190	0.256	0.463

Notes: The dependent variables in columns (3)-(6) are mother's education years and age at birth and father's education years and age at birth, respectively. An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.9: The impact of high temperatures during pregnancy on cognitive ability including county specific survey month fixed effects

	(1)	(2)
Dependent Variable:	Word Test	Math Test
High Temp Days	-0.0019**	-0.0010
	(0.0009)	(0.0009)
Low Temp Days	0.0005	0.0007
	(0.0007)	(0.0006)
Precipitation (log)	-0.0037	0.0074
	(0.0160)	(0.0151)
Female	-0.2971***	-0.3292***
	(0.0332)	(0.0331)
Han	0.0560	0.0080
	(0.1155)	(0.1109)
Mother's Education Years	0.0176***	0.0244***
	(0.0027)	(0.0032)
Mother's Age at Birth	0.0025	0.0084***
	(0.0030)	(0.0027)
Father's Education Years	0.0292***	0.0353***
	(0.0029)	(0.0031)
Father's Age at Birth	-0.0011	-0.0054**
	(0.0023)	(0.0025)
Birth Order	0.0017	0.0006
	(0.0108)	(0.0101)
Number of Siblings	0.0055	-0.0014
	(0.0097)	(0.0093)
County-Survey Month FE	Yes	Yes
Birth Month FE	Yes	Yes
Province-Year FE	Yes	Yes
Observations	10685	10685
R-Squared	0.451	0.430

Notes: An observation is an individual born in a rural area. The dependent variable are standardised word-test and math-test scores. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.10: The impact of high temperatures during pregnancy on personal annual income (Log 2010 CNY)

	(1)	(2)	(3)	(4)
Dependent Variable:	Income(ln)	Income(ln)	Income(ln)	Income(ln)
High Temp Days	-0.0006	-0.0008	-0.0009	-0.0013
	(0.0013)	(0.0013)	(0.0012)	(0.0015)
Low Temp Days	-0.0009	-0.0002	-0.0002	0.0006
	(0.0010)	(0.0009)	(0.0009)	(0.0011)
Precipitation (log)	0.0221	0.0100	0.0114	0.0381
	(0.0173)	(0.0170)	(0.0153)	(0.0241)
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes	Yes
County Specific Linear Trend	No	No	Yes	No
County Specific Quadratic Trend	No	No	Yes	No
Province-Year FE	No	No	No	Yes
Observations	8077	8077	8077	8077
R-Squared	0.202	0.280	0.317	0.379

Notes: An observation is an individual born in a rural area. The dependent variable is the logarithm of personal annual income. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.11: The impacts of high temperatures on all outcomes (county-year fixed effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0104***	-0.0025**	-0.0015	-0.0082	-2.5492**	0.0015**
	(0.0039)	(0.0010)	(0.0010)	(0.0054)	(1.0849)	(0.0007)
Low Temp Days	0.0057*	0.0006	0.0009	0.0043	-1.4540	0.0002
	(0.0033)	(0.0008)	(0.0008)	(0.0052)	(0.9054)	(0.0006)
Precipitation (log)	-0.0454	-0.0257	-0.0183	-0.1111	-158.2645***	0.0687**
	(0.1286)	(0.0356)	(0.0328)	(0.2059)	(53.2945)	(0.0344)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10685	10685	10685	10685	3223	3223
R-Squared	0.591	0.590	0.581	0.703	0.685	0.634

Notes: An observation is an individual born in an rural area. High-temperature days are defined as those with daily maximum temperature higher than 85°F. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.12: The impacts of high temperatures during pregnancy on adult outcomes and birth weight for urban-born individuals

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0032	0.0013	0.0018	-0.0006	2.1692	-0.0002
	(0.0081)	(0.0018)	(0.0017)	(0.0154)	(2.3643)	(0.0010)
Low Temp Days	-0.0034	-0.0031	-0.0015	0.0224	-1.3228	0.0006
	(0.0060)	(0.0012)	(0.0013)	(0.0122)	(2.0120)	(0.0008)
Precipitation (log)	-0.0781	-0.0092	0.0103	-0.6436	-106.0663	0.0715
	(0.2835)	(0.0812)	(0.0645)	(0.6065)	(122.5447)	(0.0443)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2314	2314	2314	2314	1198	1198
R-Squared	0.676	0.617	0.622	0.768	0.604	0.594

Notes: An observation is an individual born in an urban area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.13: Does a high proportion of heat-tolerant crops mitigate the adverse effects of high temperatures during pregnancy on all outcomes?

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days	-0.0096**	-0.0031***	-0.0019*	-0.0097	-1.5064	0.0014*
	(0.0041)	(0.0010)	(0.0010)	(0.0065)	(1.2999)	(0.0008)
High Temp Days X C4	0.0100	0.0082	0.0047	-0.0166	-5.9332	-0.0024
	(0.0220)	(0.0059)	(0.0053)	(0.0365)	(8.0442)	(0.0049)
Low Temp Days	0.0055**	0.0011	0.0012*	0.0037	-0.7229	-0.0000
	(0.0026)	(0.0007)	(0.0007)	(0.0047)	(0.9130)	(0.0004)
Precipitation (log)	0.0724	-0.0039	0.0040	-0.1863*	-62.5677*	0.0329*
	(0.0705)	(0.0150)	(0.0144)	(0.0995)	(33.7894)	(0.0196)
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10660	10660	10660	10660	3219	3219
R-Squared	0.403	0.416	0.395	0.576	0.414	0.322

Notes: An observation is an individual born in a rural area. C4 Plant Area represents corn and sugarcane proportion of crop acreage within the province. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. 25 observations are missing from the main regression sample, because crop-area information is missing for Shanghai in 1993. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

Table A1.14: The impacts of high temperatures during nine months after birth on adult outcomes and birth weight

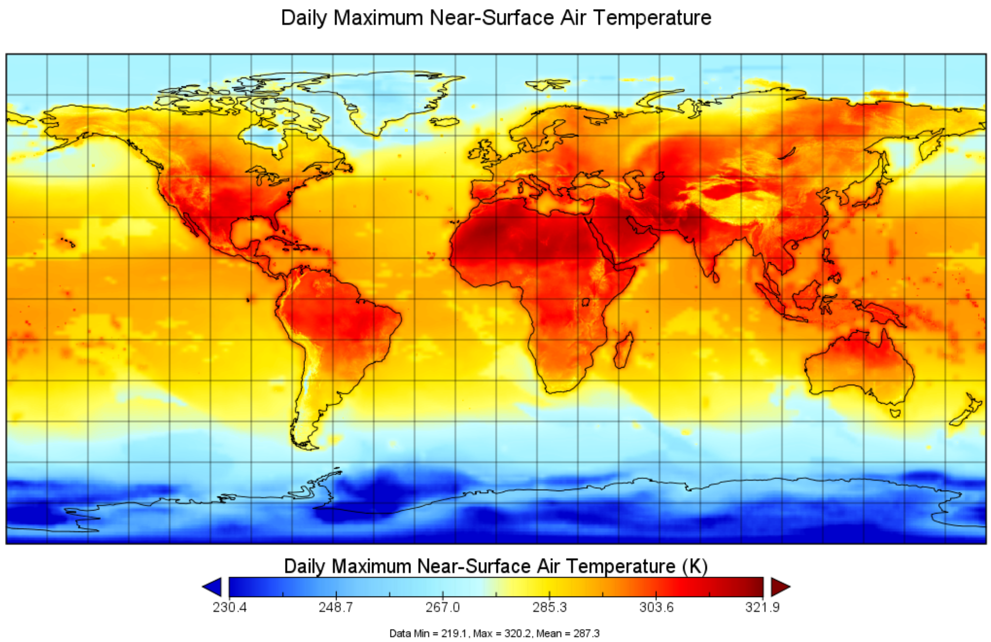
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Birth Weight	LBW	Eduy	Word Test	Math Test	Height
High Temp Days (9 Mon After Birth)	-0.7784	0.0004	-0.0025	-0.0011	-0.0003	-0.0114**
	(1.1840)	(0.0007)	(0.0038)	(0.0009)	(0.0008)	(0.0048)
Low Temp Days (9 Mon After Birth)	-0.6713	0.0007	-0.0012	0.0001	-0.0002	0.0002
	(0.9099)	(0.0005)	(0.0032)	(0.0007)	(0.0007)	(0.0041)
Precipitation (9 Mon After Birth, log)	0.1181	-0.0001	0.0000	-0.0001	0.0000	0.0000
	(0.0741)	(0.0000)	(0.0002)	(0.0001)	(0.0001)	(0.0003)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3223	3223	10685	10685	10685	10685
R-Squared	0.413	0.322	0.403	0.416	0.396	0.576

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.

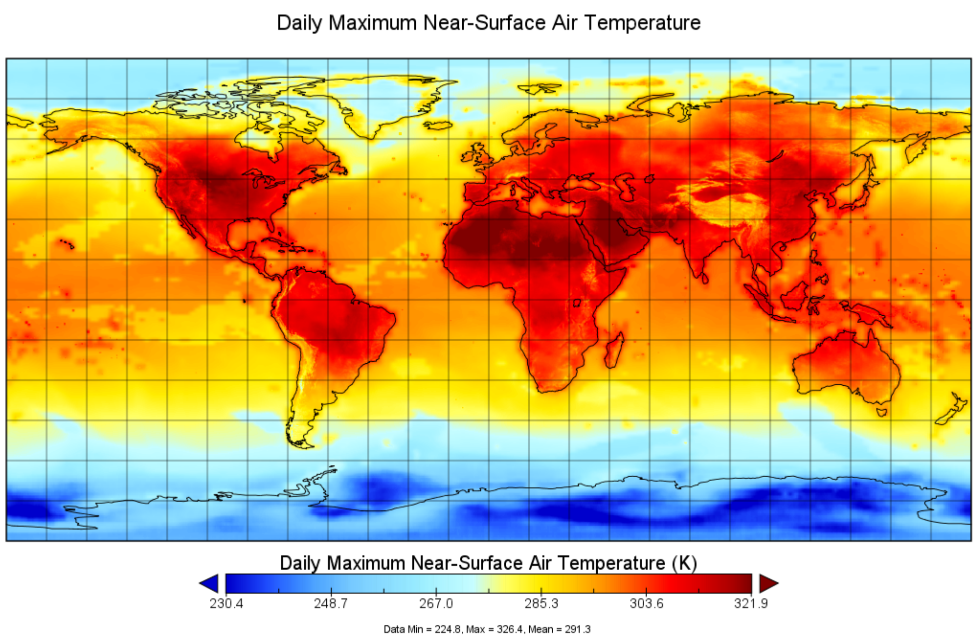
Table A1.15: The impacts of high temperatures during six months before conception on adult outcomes and birth weight

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Eduy	Word Test	Math Test	Height	Birth Weight	LBW
High Temp Days (6 Mon Before Conception)	0.0016	0.0008	-0.0000	0.0070	0.4026	-0.0006
	(0.0033)	(0.0007)	(0.0007)	(0.0043)	(0.9983)	(0.0006)
Low Temp Days (6 Mon Before Conception)	0.0002	0.0001	0.0002	0.0009	0.9232	-0.0004
	(0.0025)	(0.0005)	(0.0005)	(0.0037)	(0.7127)	(0.0004)
Precipitation (6 Mon Before Conception, log)	-0.0001	0.0000	-0.0000	-0.0002	-0.0710	-0.0000
	(0.0002)	(0.0001)	(0.0000)	(0.0004)	(0.0693)	(0.0000)
Demographics Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10601	10601	10601	10601	3212	3212
R-Squared	0.405	0.417	0.396	0.577	0.413	0.321

Notes: An observation is an individual born in a rural area. High- and low-temperature days are defined as those with daily maximum temperatures higher than 85°F and with daily minimum temperatures lower than 32°F. Six months before conception is defined as ten to fifteen months before birth. Demographic controls include gender, race, birth order, number of siblings, and parents' education years and age at delivery. Ordinary least squares estimates. Standard errors in parentheses, clustered by county. ***Significant at 1%, **significant at 5%, *significant at 10%.



(a) 2000



(b) 2100

Figure A1.1: The global daily maximum near-surface air temperature on the 1st July, 2000 (panel a) and 2100 (panel b).

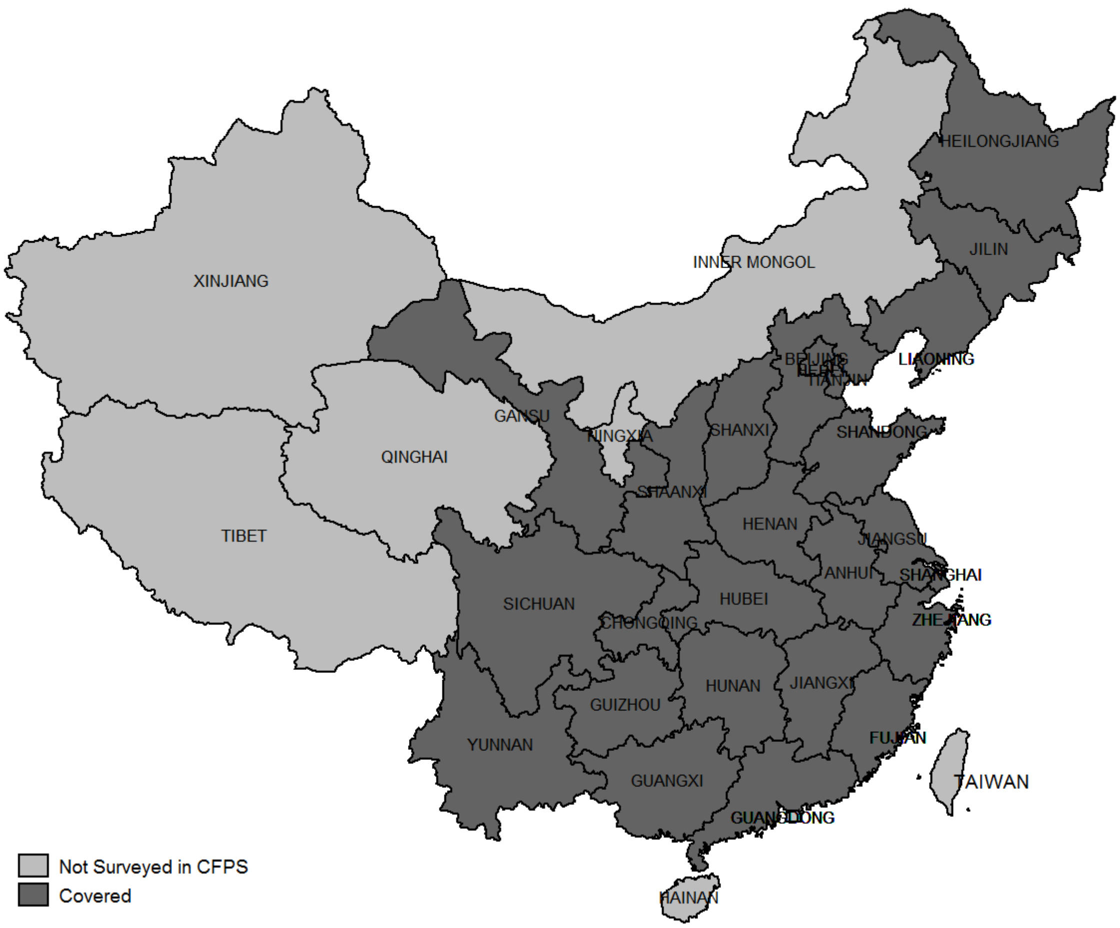
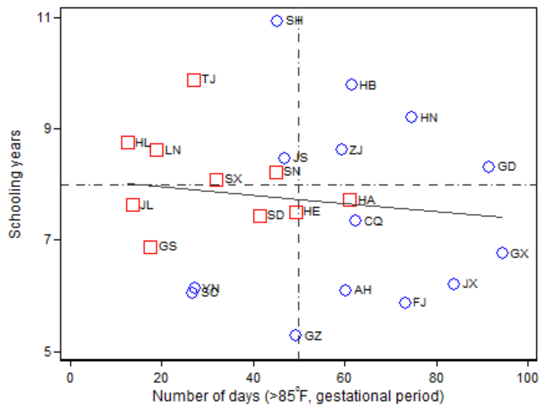
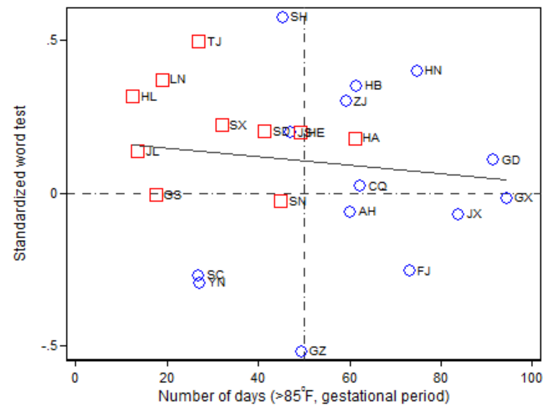


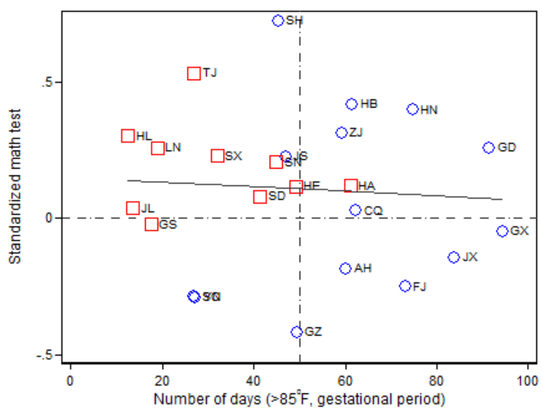
Figure A1.2: Provinces covered in the CFPS sample.



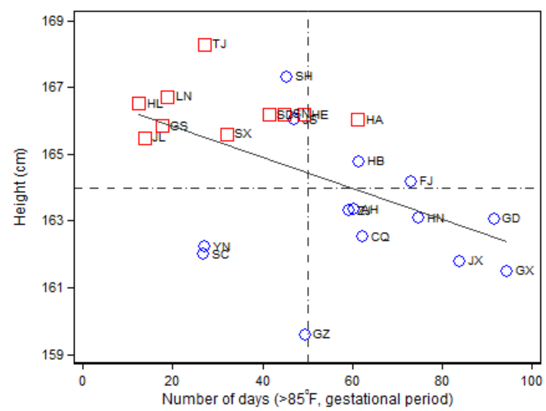
(a) Education years



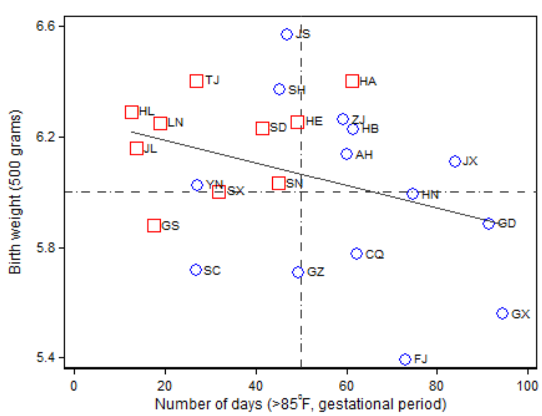
(b) Word-test score



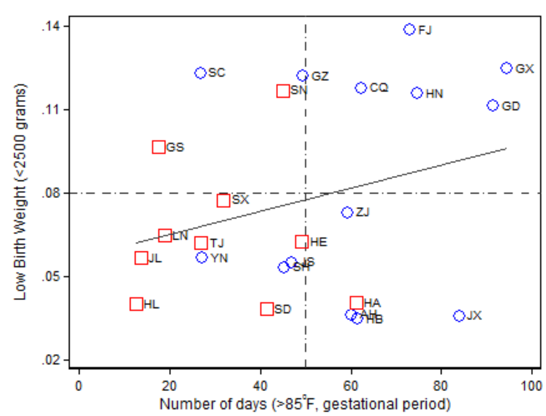
(c) Math-test score



(d) Height



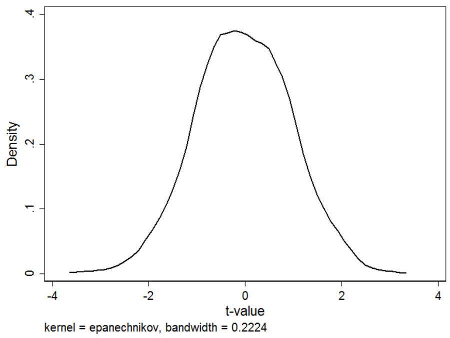
(e) Birth weight



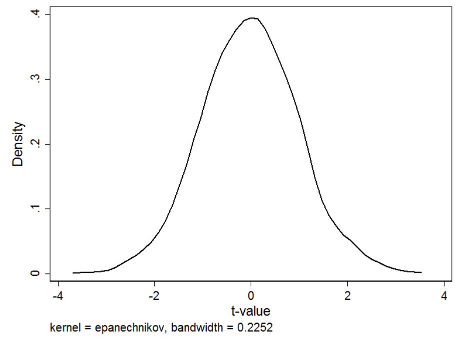
(f) LBW

Figure A1.3: Adult outcomes and birth weight against number of high-temperature days (>85°F) for typical gestational period by province.

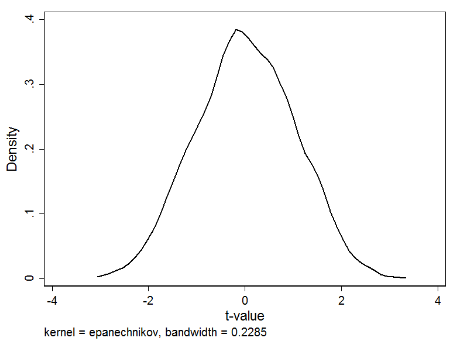
Notes: Red square and blue circle markers represent provinces in the north and south, respectively. The solid line is fitted using OLS.



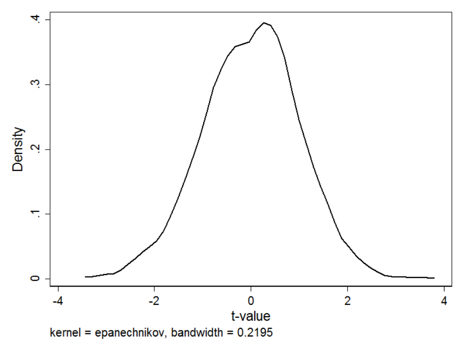
(a) Schooling years



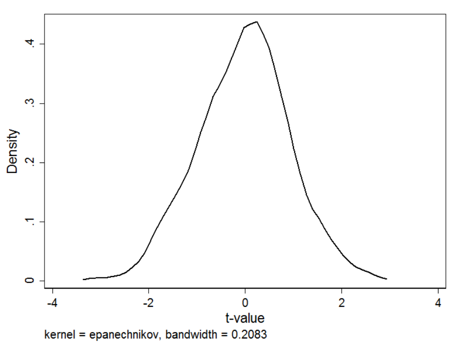
(b) Word-test score



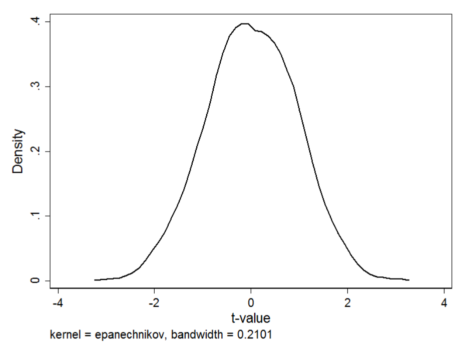
(c) Math-test score



(d) Height

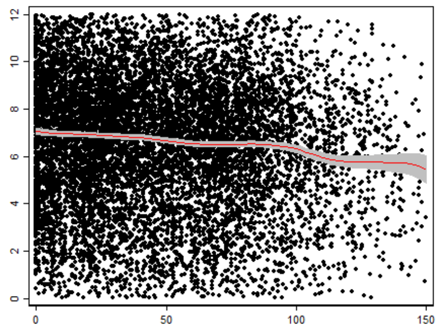


(e) Birth weight

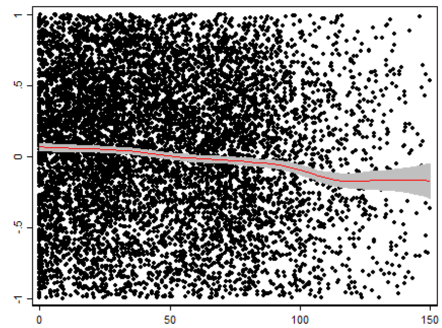


(f) LBW

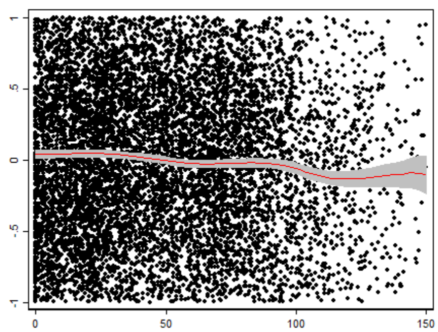
Figure A1.4: Distribution of t-statistic for high-temperature days of 1,000 estimates for Equation (1) with placebo birth time and place.



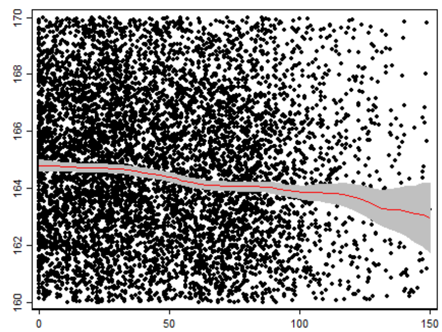
(a) Schooling years



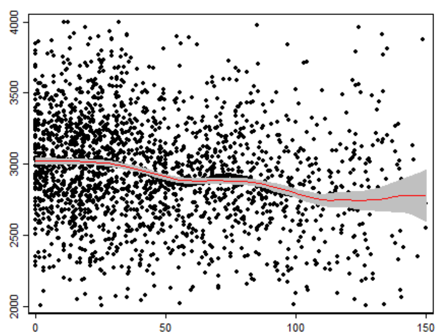
(b) Word-test score



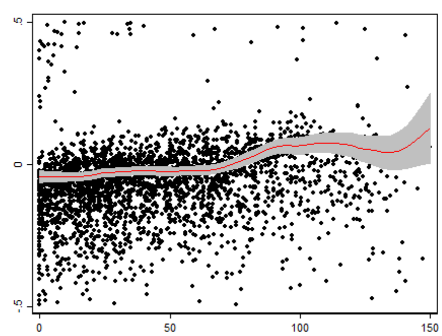
(c) Math-test score



(d) Height



(e) Birth weight



(f) LBW

Figure A1.5: High-temperature days (>85°F) during pregnancy against adult outcomes and birth weight.

Notes: The solid line shows the fitted partially linear model, and the gray area denotes the 95% confidence interval.

Chapter 2

The NO_x Budget Trading Program, Air Pollution, and Criminal Activities

2.1 Introduction

Air pollution is considered to be an environmental stressor associated with increases in physiological and psychological symptoms (Campbell 1983). Previous studies have provided strong evidence that air pollution affects human well-being in many aspects—e.g., infant mortality (Chay and Greenstone 2003; Currie and Neidell 2005); life expectancy (Chen et al. 2013); worker productivity (Zivin and Neidell 2012); academic performance (Ebenstein et al. 2016); and so forth. We explore a new dimension—how criminal activities are driven by air pollution—in the present study. Epidemiological literature shows that poor air quality can cause people to behave aggressively due to anxiety, tension, anger, or depression, which suggests that air pollution may be associated with violent crimes (e.g., Rotton 1983).[1] In this study, we employ a well-known quasi-experiment—the NO_x Budget Trading Program (hereafter NBP)—to identify the causal effects of air pollution (pollution emissions) on criminal behaviors.

The NBP was a cap-and-trade system aimed at reducing ozone concentrations.[2] It was initiated in 2003 and ended in 2008. As ozone concentrations are generally high in the summer, the NBP only operated from May to September.[3] Nineteen Eastern and Midwestern states, together with Washington, DC, were included in this program (see Figure 2.1). Therefore, this quasi-experiment provides three dimensions of variations. The first is the difference in criminal activities between NBP and non-NBP states. The second difference arises from before versus after the market's initiation, and summer versus winter is the last dimension. Employing these three sources of variation, we use a triple-difference estimator to examine the relationship between air pollution and criminal acts.

By compiling county-season-level crime data with pollution emission and weather information, we find that the NBP market statistically significantly lowered violent crimes in participating states. To be specific, rates for rape and robbery statistically significantly decreased by 3.8 and 4.7 percent, respectively. Although the NBP's effects on murders and assaults are not statistically significant at the traditional level, the magnitudes are not

[1] Possible mechanisms that explain the relationship between air pollution and criminal activities are detailed in Section 2.

[2] Details about the NBP market are provided in Deschenes et al. (2012) and Curtis (2014).

[3] 2004 is an exception. The NBP operated from June to September in that year.

negligible; Rates for murder and assault fell by 2.1 and 1.2 percent, respectively.[4] What's more, the effects of the NBP market on property crimes are not statistically significant and the magnitudes are relatively small.

To validate the assumption of the triple-difference estimator, we use two methods to examine the presence of pre-existing trends. First, we plot the impacts of the NBP on criminal activities across years. These event-time graphs show that before the market's initiation, there were no meaningful differences in the trend in summertime criminal activities between participating and non-participating states. As the event-time-study method requires large samples to get the precisely estimated effect for each year, one concern might be that the statistically insignificant differences in the 1998-2002 period are due to the lack of statistical power. To address this concern, we conduct another pre-existing trend test. Instead of separately estimating the coefficient for each year, we allow NBP and non-NBP states to have their own linear trends. Again, we find no clear pre-existing trends in our triple-difference setting.

Our study contributes to the environment economics literature in several ways. First, this paper, along with a concurrent study by Herrnstadt et al. (2016), is the first to identify the causal relationship between air pollution (pollution emissions) and criminal activities. Estimating this relationship is challenging, because it is confounding from economic activities that may bias standard estimates. For perspective, local economic activities not

[4]Overall violent crimes declined by 1.4 percent. To put this figure into perspective, Chalfin and McCrary (2013) found that violent crimes decrease by around 0.4 percent once police officers increase by 1 percent. Based on our estimate, the effect of the NBP on violent crimes is equivalent to increasing the size of the police force by 3.5 percent during the sample period.

only affect criminal acts (e.g., Raphael and Winter-Ebmer (2001); Gould et al. (2002)), but are also related to air pollution concentrations. Another challenge is that measurement errors in assigning pollution emission monitors to counties shrink estimates towards zero. Without considering the endogeneity problem of pollution emissions, our fixed-effect estimates indicate that air pollution does not drive any criminal behaviors—but the instrumental variable estimates demonstrate that air pollution (pollution emissions) is indeed a determinant of violent crimes.

Previous psychological studies have also examined the association between air pollution and criminal activities. Strahilevitz et al. (1979) found that psychiatric disturbances increased as air pollution levels went up. Rotton and Frey (1985), using archival data for Dayton, Ohio, documented that family disturbances and assaults were affected by ozone, smoke, and meteorological factors. On the other hand, using cross-sectional data, Lave and Seskin (1978) did not find any significant relationships between outdoor air pollution and crimes in the U.S.—e.g., rapes, robberies, assaults, burglaries, and auto thefts. However, these studies either are based on cross-sectional data or employ a small set of controls. As a result, they have limited ability to address endogeneity issues.

Second, we exploit the seasonal variations of crime and show the relatively long-term effects of air pollution (pollution emissions) on criminal activities. To compare, Herrnstadt et al. (2016) exploited daily variations in air pollution and violent crimes in Los Angeles and Chicago and found that air pollution statistically significantly increased violent crimes. Jacob et al. (2007), in contrast, found that crime rates were negative serial correlated over a span of weeks. Therefore, the daily link between air pollution and criminal activities may not be capable of reflecting the long-term effects (Ranson 2014).[5] Last, our findings provide evidence of the potential benefits of pollution emission reduction, and thus have important policy implications.

The rest of the paper is organized as follows. The second section describes mechanisms that may explain the association between air pollution and criminal activities. Section 3 introduces the empirical framework. Section 4 summarizes the data sources and presents the descriptive analysis. The main findings and sensitivity analysis are presented in Section 5, and Section 6 presents implications of our findings and concludes.

2.2 Possible mechanisms

In this section, we summarize the potential mechanisms proposed in psychological, biological, and economics literature that may support the relationship between air pollution and criminal activities.

First, based on laboratory experiments, researchers have found that a number of negative psychological symptoms are associated with air pollution—e.g., anxiety, tension, anger, and depression (for instance, Evans et al. 1987; Zeidner and Shechter 1988). These symptoms may directly influence human judgment and may be reflected explicitly as

[5]We acknowledge that even using seasonal data, temporal substitution may also bias our estimates. In other words, individuals may change their crime timing from summer to winter. To tackle this problem, we aggregate data to the county-year level and estimate the policy effect on crime using a difference-in-differences method. In Table A2.1, we find that our main results remain stable.

human aggression. In a laboratory study by Rotton et al. (1979), individuals who were exposed to unpleasant odors delivered higher levels electric shocks, on average, to their confederates as punishment for making errors on a learning task, compared to their counterparts under clean air. Similarly, Jones and Bogat (1978) found that people who were exposed to secondary cigarette smoke behaved more aggressively.

Second, the respiratory system is well documented to be directly affected by air pollution. For instance, oxidative stress responses have been consistently observed when people were exposed to ozone pollution (Chuang et al. 2007; Corradi et al. 2002; Valavanidis et al. 2013). In addition, air pollution is linked with neuro-inflammation (Block and Calderón-Garcidueñas 2009; Levesque et al. 2011). Both oxidative stress and neuro-inflammation can cause anxiety and have possible links with aggressive behaviors (Rammal et al. 2008).

Third, as argued by Ranson (2014), environmental factors may play a role in Becker's (1968) production function for crime. In the canonical model of crime, Becker suggested that implementation of criminal activities is based on the benefits and costs. Air quality conditions may change the benefits and costs. For perspective, when air pollution is high, police officers may choose to stay indoors instead of patrolling, thereby increasing the probability of successfully committing a crime and escaping undetected.[6]

To summarize, the first two strands of literature indicate that air pollution may increase

[6]For perspective, Zivin and Neidell (2012) found that the productivity of outdoor workers are negatively impacted by air pollution. Hanna and Oliva (2015) presented evidence that air pollution reduces labor supply. Likewise, police officers' labor supply and "productivity" may also be negatively affected by pollution.

violent, but not property, crimes. The third explanation suggests that police officers may be less productive under poor air quality, in which case both violent and property crimes would increase. According to the possible mechanisms, in this study we expect that the NBP reduces violent crimes; property crimes may be affected as well.

2.3 Empirical framework

As discussed above, the quasi-experiment—the NBP—provides three dimensions of variations in pollution emissions and criminal activities. Specifically, the first is to contrast the periods before and after the program's operation. The NBP started in 2003 and covered eight states and Washington, DC. Another 11 states joined in 2004. Participating versus non-participating states is the second dimension, and the third dimension is the NBP's operating season, i.e., from May 1 to September 31.

2.3.1 Main specification

To isolate the causal effects of the emission market on criminal activities, we employ the triple-difference (DDD) specification, similar to that of Deschenes et al. (2012). In particular:

$$Y_{ist} = \beta \mathbf{1}(DDD)_{ist} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \epsilon_{ist}. \tag{2.1}$$

where i, s, and t denote county, season, and year, respectively. Two seasons—summer and winter—constitute a year.[7] The NBP only operated in summer, during which ozone pollution generally remains high. The dependent variables, Y_{ist}, are the log of total number of criminal activities per 1,000 people in each county-year-season cell, including murders, rapes, robberies, assaults, burglaries, larcenies, and motor vehicle thefts. The variable of interest, $\mathbf{1}(DDD)_{ist}$, is defined as follows: When a state participated in the NBP in 2003 (or 2004), we set $\mathbf{1}(DDD)_{ist} = 1$ for all counties in that state in summertime in 2003 (or 2004) through 2008.

As meteorological factors are correlated with criminal activities, we should add them to our regressions (Ranson 2014). Following Ranson (2014), we use 11 bin indicators to model the daily distribution of average temperatures within a county-season-year cell: $(-\infty, 10°F]$, $(10, 20°F]$, $(20, 30°F]$, $(30, 40°F]$, $(40, 50°F]$, $(50, 60°F]$, $(60, 70°F]$, $(70, 80°F]$, $(80, 90°F]$, $(90, 100°F]$, and $(100, +\infty°F)$. Precipitation is divided into four categories: 0mm, $(0, 5mm]$, $(5, 15mm]$, and $(15, +\infty mm)$. Dew point temperature covers nine groups: $(-\infty, 10°F]$, $(10, 20°F]$, $(20, 30°F]$, $(30, 40°F]$, $(40, 50°F]$, $(50, 60°F]$, $(60, 70°F]$, $(70, 80°F]$, and $(80, +\infty°F)$.

Three sets of two-way fixed effects are further added to the main specification, i.e., county-year (μ_{it}), season-year (λ_{st}), and county-season fixed effects (η_{is}). First, county-year fixed effects capture nonlinear changes in the determinants of criminal activities within a county-year cell, e.g., the local unemployment rate and police officer recruit-

[7] In 2004, summertime is from June to September. In other years, summertime is from May to September.

ment. Second, by controlling year-by-season fixed effects, we partial out common shocks across season by year, e.g., summer vacation and the Christmas holiday (McDowall et al. 2012; Miron 1996). Third, county-specific seasonality patterns of criminal behaviors are controlled by county-season fixed effects. ϵ_{ist} denotes an idiosyncratic random error term. To allow for potential temporal and spatial autocorrelations, standard errors are clustered at the state-season level.[8]

2.3.2 Identification assumption

The validity of this triple-difference estimator relies on the parallel trend assumption. In this context, it requires that without the policy intervention, NBP and non-NBP states have the same trends on seasonal differences of criminal activities. The following specification, which was also applied by Deschenes et al. (2012), tests this assumption:

$$Y_{ist} = \sum_{t=1998}^{2008} \beta_t \mathbf{1}(NBP\ State\ and\ Summer)_{is} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \epsilon_{ist}. \quad (2.2)$$

Here, for all summer-NBP observations for all years, $\mathbf{1}(NBP\ State\ and\ Summer)_{is} = 1$. Other notations are the same as those in Equation (1). In practice, we set 2002 as the omitted group, i.e., $\beta_{2002} = 0$. If coefficients for 1998 through 2001 are not statistically significantly different from zero, this may imply that there is no evidence of clear differences in the trend of criminal acts in summertime between NBP and non-NBP states

[8] The appendix also reports standard errors that allow for autocorrelation within the state-year or state level. As shown in Tables A2.2 and A2.3, our main conclusions remain unchanged.

before 2003. In addition to assessing the common trend assumption, this specification can estimate the policy effect for each year after the market's operation as well.

One potential flaw in this method is that it requires large samples to get the precisely estimated effect for each year; Otherwise, insignificant effects may be due to the lack of statistical power. To overcome this problem, we employ the following specification to double check the common trend assumption:

$$\Delta Y_{it} = \rho_1 \mathbf{1}(NBP)_i * t + \sigma_i + \tau_t + \xi_{it} \tag{2.3}$$

where ΔY_{it} represents the differences in criminal activities between summer and winter within a county-year cell. $\mathbf{1}(NBP)_i$ is a dummy indicating those counties in the NBP states. County and year fixed effects are denoted by σ_i and τ_t, respectively. Using the data from 1998 to 2002, we test whether there is a significantly different pre-trend between the NBP and non-NBP states. The null hypothesis is $\rho_1 = 0$. Standard errors are clustered at the state level.

2.3.3 Instrumental variable estimation

Next, we use DDD in Equation (1) as an instrument variable to measure the effects of NO_x emissions on criminal behaviors. Specifications for the two-stage-least-square estimation are as follow:

$$\textbf{First stage}: NOx_{ist} = \beta\mathbf{1}(DDD)_{ist} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \epsilon_{ist}. \tag{2.4}$$

$$\textbf{Second stage}: Y_{ist} = \alpha\widehat{NOx_{ist}} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \epsilon_{ist}. \tag{2.5}$$

where $\widehat{NOx_{ist}}$ is the predicted NO_x emissions within a county-year-season cell from the first stage. Other notations are the same as those in Equation (1). To measure each county's NO_x emissions, we assign the pollution emissions monitoring facility closest—i.e., less than 50 km—to each county. Counties without a monitoring facility within 50 km are excluded.

The IV estimation requires the validity of the exclusion restriction assumption, i.e., $\mathbf{1}(DDD)_{ist}$ affects criminal activities only through NO_x emissions. As can be seen in Table A2.4, NO_x emissions decreased by about 33.1% of 1997-2002 mean summer emissions in NBP states. Although reductions in SO_2 emissions are statistically significant, the effect is relatively small (see Table A2.5). Another concern about the validity of the exclusion restriction assumption is that local economic conditions may also be affected by the NBP market. For instance, Curtis (2014) found that unemployment in the manufacturing sector increased as a result of the NBP market. Given these concerns, we emphasize that the instrumental variable estimates in this paper should be cautiously interpreted.[9]

[9]As the NBP decreased not only ozone but also NO_2 and $PM_{2.5}$ concentrations (Deschenes et al. 2012), using ozone alone as the endogenous variable to measure the relationship between ozone and criminal activities may not be appropriate.

Nevertheless, the IV estimates provide important policy implications.

2.4 Data and descriptive analysis

2.4.1 Data sources

To assess the impacts of air pollution on criminal activities, we compile a rich set of data on crime, pollution emissions, and meteorology for the period 1998-2008.[10]

Crime data. Crime data are extracted from the Uniform Crime Reporting (UCR) Program operated by the US Federal Bureau of Investigation (FBI). Based on around 17,000 local enforcement agencies' monthly reports, the data cover about 3,000 counties in the 49 continental states, representing 97.4% of the US population (Federal Bureau of Investigation 2011). Data for criminal activities are submitted voluntarily by city, county, and state law enforcement agencies. The FBI is responsible for checking the completeness and accuracy of the reports. If the FBI detects an unusual fluctuation in an agency's criminal activities, it will contact the local enforcement agency to explain or correct the figures. Therefore, the UCR data should contain fewer errors.

Monthly reports typically include the number of two types of reported offenses—violent and property crime. Specifically, the violent crime includes murders, rapes, robberies, and assaults. Burglaries, larcenies, and motor thefts constitute the property crime.[11]

[10]Our conclusions are not sensitive to the choice of the start year of the sample period (see Figure A2.1).

[11]More detailed criminal activities are also provided in the dataset. For instance, there are four categories of robberies, i.e., firearm; knife or cutting instrument; other dangerous weapon; and strong-arm robberies.

In the dataset, a number of counties report criminal activities only once, twice, or four times a year.[12] To maintain our sample balance, these observations are deleted. What's more, in some cases, agencies provide only the total number of violent and property crimes instead of the number for each type of offense. We drop these cases as well. Furthermore, we eliminate all records in which total criminal activities is zero for every month within a year.[13] Following Ranson (2014), we drop all county-year cells with a population of fewer than 1,000 people.

Pollution emissions. Pollution emissions data are obtained from the EPA's Clean Air Markets Division. Firms in the NBP report pollution emissions only during the summer, i.e., from May 1 to September 30.[14] Such data availability constrains us from comparing summer versus winter. However, another pollution reduction program, called the Acid Rain Program (ARP), provides firms' emissions data for the entire year. Almost all the firms enrolled in the NBP are also in the ARP; it provides total daily emissions of NO_x, SO_2, and CO_2 for 1,734 firms in 645 counties in the 49 states. As those firms are enrolled in the cap-and-trade market and monitored by the EPA, the measurements on pollution emissions are supposed to have fewer errors (Deschenes et al. 2012).

Weather data. Weather data are provided by the National Oceanic and Atmospheric Administration (NOAA) and include 1,761 different weather stations across the U.S. The weather variables include daily average temperature, total daily precipitation, and

[12]Most cases are from counties in Alabama and Florida.
[13]Most of these cases are in Illinois counties.
[14]One exception is that in 2004, the NBP initiated from the end of May.

dew point temperature. To ensure the accuracy of weather readings, we select all weather stations that are less than 50 km from the county's centroid to construct the meteorological variables, using a inverse-distance-weighted average. The use of alternative distance thresholds, such as 100 km and 150 km, does not change our main results.[15]

In the analysis, we exclude non-continental states—i.e., Alaska and Hawaii—and Puerto Rico. As Deschenes et al. (2012) argued, states adjacent to NBP states were also likely to benefit from pollution reduction, given that air pollution can cross state borders. These states—Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, Vermont, and Wisconsin—are also excluded.[16] Moreover, only certain counties in Michigan participated the NBP, and we do not include it. Sample statistics are summarized in Table 2.1. The final sample contains 1,737 counties in 37 states.

2.4.2 Descriptive analysis

To estimate the relationship between pollution emissions and criminal activities, we begin with a preliminary analysis in which NO_x emissions are treated as exogenous. In other words, we estimate Equation (4) directly by a fixed-effect model instead of instrumenting NO_x emissions with an exogenous policy change. The specification is as follows:

$$Y_{ist} = \alpha NOx_{ist} + W^{'}_{ist}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \epsilon_{ist}. \tag{2.6}$$

[15]Due to limited space, results are not included but are available on request.

[16]In sensitivity analysis, we include these states as the control or treatment group. Our results remain stable (see Table A2.6 and A2.7).

where NOx_{ist} denotes NO_x emissions in each county-year-season cell. Other notations are the same as those in Equation (4).

Table 2.2 statistically reports fixed-effect estimates of the effects of NO$_x$ emissions on criminal activities. Columns (1) to (4) in Table 2.2 show that after partialling out county-by-season, season-by-year, and county-by-year fixed effects and flexible meteorological factors, the effects of NO_x emissions on violent crimes are statistically indifferent from zero. Although the signs of most coefficients are positive, the magnitudes are too small, which suggests that NO_x emissions have little influence on violent crimes. Similarly, columns (5) through (7) in Table 2.2 present estimates on property crimes. The effects on burglaries and motor vehicle thefts are not statistically significant from zero, and although the estimate on larcenies is statistically significant at the 10% level, the magnitude is not large.

To summarize, the fixed-effect estimates provide little evidence that NO_x emissions have a strong relationship with criminal activities. However, these estimates could suffer from omitted variable bias, even though we have controlled three sets of two-way fixed effects. In Equation (5), after partialling out county-by-season, season-by-year, and county-by-year fixed effects, variations are at the county-year-season level. At this variation level, local economic conditions may still be an important omitted variable that is closely related to both criminal activities and pollution emissions; it may bias our estimates towards zero. Another possibility is that measurement errors in assigning pollution emission monitors to counties shrink the fixed-effect estimates towards zero. Therefore, to correct the bias we employ an exogenous policy change—the NBP—that is supposed to be unrelated to local economic conditions.[17] We expect that by using IV methods, the coefficients of NO_x emissions would become larger if air pollution did affect criminal behaviors.

2.5 Main results

This section first summarizes estimates of the reduced-form effects of the NBP on violent and property crimes. Next, we employ two methods to check the validity of the identification assumption for this triple-difference setting. In addition, using an IV approach, we measure the effects of NO_x emissions on criminal activities and compare them to fixed-effect estimates. Finally, we present several sensitivity analyses.[18]

2.5.1 Violent crimes

Murder. Table 2.3 statistically presents the effect of the cap-and-trade market on murders. In column (1), we control county-by-season, season-by-year, and state-by-year fixed effects at first. The coefficient indicates that the NBP market statistically significantly decreased the murder rate in NBP counties by 2.2%. As discussed before, meteorological factors are correlated with both air pollution concentrations and criminal activities; therefore, we add flexible weather controls in the second column. Column (3) is the rich-

[17] The validity of the instrumental variables estimates is discussed in the empirical framework section.

[18] As Deschenes et al. (2012) have proved that the NBP significantly decreased NO_x emissions and ozone and NO_2 concentrations, we do not emphasize these results in this study.

est specification, in which state-by-year fixed effects are replaced with county-by-year fixed effects. We notice that although the estimate gets less precise in more restrictive specifications, the magnitude of the coefficient of interest is rather stable (around 2.1%).

Panel (a) of Figure 2.2 performs an event-time study for log of the murder rate. As Equation (2) shows, the corresponding graph displays the changes of the coefficient on the variable $\mathbf{1}(NBP\ State\ and\ Summer)_{is}$ across years. Similar to Curtis (2014) and Deschenes et al. (2012), we set 2002 as the omitted group and normalize the coefficient for 2002 to be zero. The figure shows that before the market's initiation the coefficients are positive but insignificant, which indicates that there are no clear differences in the trend in summertime murder rates between NBP and non-NBP states. Moreover, after the start of the program, coefficients gradually become close to zero.

Rape. Table 2.4 examines the impact of the NBP market on rapes. Columns (1) through (3) replicate the specifications from Table 2.3. Panel (A) of Table 2.4 shows that the NBP market decreased the rape rate significantly. Specifically, the most stringent specification, in column (3), indicates that the rape rate fell by 3.8%. The estimate is statistically significant at the 5% level. As more controls are added, the absolute value of the coefficient becomes slightly larger.

Panels (B) and (C) outline the impacts of the NBP market on two detailed types of rapes, i.e., forcible and attempted forcible rapes. By comparing the results in the two panels, we notice that the effect is much larger on forcible rapes than attempted forcible rapes. In particular, as column (3) shows, the forcible rape rate statistically significantly declined by 4.2%. The attempted forcible rape rate fell by 1.8%, but the effect is not significant at the traditional level.

The event-time study for log of the rape rate is displayed in panel (b) of Figure 2.2. It is worth noting that before the market's initiation, most coefficients across years are not statistically significantly different from zero, suggesting that the common trend assumption may hold. In addition, we find that the four out of six coefficients of the 2003-2008 period are negative. In particular, the coefficients on 2004 and 2006 are almost statistically significant at the 5% level.

Robbery. Table 2.5 reports the NBP's effects on robbery rates and two detailed types of robberies (firearm and strongarm robberies).[19] As can be seen in panel (A), the cap-and-trade market has a statistically significantly negative effect on the robbery rate. Column (3) indicates that the NBP reduced the robbery rate by 4.7% (significant at the 5% level). From columns (1) to (3), the standard error increases to some extent as the specification gets richer.

Column (2) in panel (B) shows that the firearm robbery rate statistically significantly declined by 2.5%. After replacing the state-by-year fixed effects with county-by-year fixed effects, the magnitude changes only slightly, but the estimate becomes imprecise. As indicated in column (3) in panel (C), the NBP's effect on the strongarm robbery rate is non-negligible and statistically significant. By comparing the results in panels (B) and

[19]Robberies have four categories: firearm, knife, strongarm, and other dangerous weapon. Compared to firearm and strongarm robberies, knife and other- weapon robberies happen less frequently. The NBP's effects on the two lesser robbery types are relatively small and insignificant. Due to limited space, the results for knife and other-weapon robberies are not included but are available on request.

(C), we find that the effect on the strongarm robbery rate is about three times as large as the effect on the firearm robbery rate.

Panel (c) of Figure 2.2 displays a clear pattern for the NBP's effect on the robbery rate across years. Before 2003, the coefficients for 1998 through 2002 are around zero and far from statistically significant at the traditional level, which implies that the parallel assumption holds. Since 2003, the coefficients are all negative. Specifically, the estimate on 2007 is statistically significant at the 5% level. The magnitudes show that the robbery rate fell by around 4%, which seems to be consistent with the estimates in Table 2.5.

Assault. Panel (A) of Table 2.6 summarizes the estimates on the effect of the NBP on the assault rate. Column (3), the most stringent specification, shows that the assault rate fell by 1.2%. The estimate is not precise, however, with a 1.20 t-statistic. In addition, we outline the results for two types of assaults: i.e., aggravated and simple.[20] We notice that the estimates for the two types of assaults are not statistically significant at the traditional level. The signs of the coefficients are negative, however, which is consistent with the estimates on other violent crimes.

Panel (d) of Figure 2.2 displays the event study graph for the assault rate. For the period 1998-2002, the coefficients are not statistically significantly different from zero, suggesting that there may not be a different trend for the assault rate in summertime between the NBP and non-NBP states. However, coefficients for the period 2003-2008

[20]Assaults cover five categories: firearm, knife, other dangerous weapon, aggravated, and simple. Compared to aggravated and simple assaults, the other assault types happen less frequently, and the NBP's effects on the three rates are not statistically significant. Due to limited space, the results are not provided but are available on request.

are also quite close to zero, although the signs of the coefficients for the period 2005-2008 are negative.

We summarize the above regression results as follows. As we expect, the NBP market statistically significantly reduced violent crimes. Specifically, the effects on murder, rape, robbery, and assault rates are 2.1%, 3.8%, 4.7%, and 1.2%, respectively. However, the estimates for murder and assault rates are not precise. Employing event time studies, we do not find any evidence showing meaningful differences in the trend in summertime violent crimes between participating and non-participating states before 2003.

2.5.2 Property crimes

Burglary. The NBP market's effect on the burglary rate is shown in panel (A) of Table 2.7. The coefficient in column (3) indicates that the NBP's impact on burglaries was relatively small—a reduction of 0.9 burglaries per 1,000 people—and the estimate is not statistically significant at the traditional level. Panels (B) and (C) present results for two main burglary categories (forcible and unlawful entry).[21] The forcible and unlawful entry rates fell by 0.7% and 0.6%, respectively, but the effects are not statistically significant, as can be seen in column (3) of panels (B) and (C).

Panel (e) of Figure 2.2 presents the impact of the NBP market on burglaries across years. Before 2003, the coefficients are not statistically significant at the 5% level.[22]

[21] Burglaries cover three types: forcible, unlawful, and attempted forcible entry; attempted forcible entry is the least frequent type. The NBP's impact on attempted forcible entries was negative but insignificant as well. Results are available upon request.

[22] The coefficient for 2001 is almost significant at the 5% level.

The common trend assumption for burglary rate may hold. After the market's initiation, the coefficients are not statistically significant different from zero, suggesting no clear evidence that the burglary rate declined.

Larceny. The influence of the NBP market on the larceny rate is shown in Table 2.8. Columns (1) through (3) indicate that the NBP had a negative effect on the larceny rate. As shown in the richest specification in column (3), the larceny rate fell by 1.5% for the NBP states. But, again, the estimate is not statistically significant at the 10% level.

The event time study for log of the larceny rate is exhibited in panel (f) of Figure 2.2. During the period 1998-2002, the coefficients are close to zero, indicating no meaningful differences in the trend in summertime between the NBP and non-NBP states. Although all coefficients in 2003 through 2008 are negative, the magnitudes are small and the estimates are not statistically significant at the 5% level.

Motor vehicle theft. Panel (A) of Table 2.9 reports reduced-form effects of the NBP market on motor vehicle thefts. Column (3) shows that the NBP's effect on motor vehicle theft rate is not statistically significantly different from zero. The sign of the coefficient is even positive. Additionally, we present the results for two types of motor vehicle thefts—auto and trucks and buses—in panels (B) and (C).[23] We notice that all coefficients are statistically insignificant, and signs and magnitudes are not stable.

Panel (g) of Figure 2.2 exhibits the impact of the NBP market on motor vehicle thefts

[23] Auto, trucks and buses, and other motor vehicle thefts constitute the motor vehicle thefts. Other motor vehicle thefts is the smallest category, and the NBP's effect on this type is not statistically significant either. Results are available on request.

across years. Similar to the patterns for other property crimes, the pattern for motor vehicle thefts shows that in advance of the NBP's initiation, we cannot reject the parallel trend assumption. After 2003, the coefficients fluctuate around zero. There is no strong evidence that the motor vehicle theft rate fell.

To summarize, in this section we find that the effects of the NBP market on property crimes are relatively small and statistically insignificant. The results indicate that the third possible mechanism we outline in the second section may not be an important one.[24] Using event graph studies, no clear evidence demonstrates any meaningful differences in the trend in summertime property crimes between participating and non-participating states before the market's initiation.

2.5.3 Validity of the identification assumption

Validity of the triple-difference estimator requires that in the absence of the NBP, the difference in criminal activities between the treatment—NBP states in summertime—and the control group is constant over time. We first employ Equation (2)—the event time study—to check trends both in advance of and after the market's initiation. As we find in the preceding subsections, Figure 2.2 indicates the absence of more than a slight pre-existing trend between NBP and non-NBP states before the market's initiation. However, the event-time-study method requires large enough samples to get precisely estimated effects for each year. In other words, the statistically insignificant coefficients in the

[24]The third mechanism is that air pollution may increase the probability of successfully committing a crime and escaping undetected.

1998-2002 period may be due to the lack of statistical power.

Next, we provide another pre-existing trend test using Equation (3). In this model, instead of separately estimating the coefficient for each year, we combine data from 1998 to 2002 and test whether there is a significantly different linear trend between the NBP and non-NBP states before 2003. Again, the null hypothesis is $\rho_1 = 0$.

As Table 2.10 shows, most estimates on the pre-existing differences between NBP and non-NBP states before 2003 are far from statistically significant, consistent with the event time studies. One point worth noting is that some pre-existing differences seem to exist in the rape rate; However, the estimate is only statistically significant at the 10% level. In general, we conclude that only a small pre-existing trend, if any, presents in our triple-difference setting.

One concern about this method is that changing the sample period may change the conclusions. To solve the problem, we present estimates based on a different sample period, i.e., using an alternative start year for the sample period. Estimates based on the 1999-2002, 2000-2002, and 2001-2002 periods are displayed in Tables A2.8, A2.9, and A2.10. Again, no clear evidence demonstrates any meaningful differences in the trend in summertime criminal activities between participating and non-participating states before the market's initiation.

2.5.4 IV estimates

In this part, we first estimate the NBP's effects on pollution emissions, which would enable us to check the validity of the IV estimation. As can be seen below, our results replicate those of Deschenes et al. (2012). Next, we employ two-stage-least-square estimation to measure the effects of NO_x emissions on criminal activities. Furthermore, we compare the IV results to fixed-effect estimates.

Table A2.4 statistically reports the NBP's effect on NO_x emissions. Compared to the average emissions in NBP states in advance of the market's initiation, NO_x emissions statistically significantly fell by about 33.1%. The magnitude is similar to that of Deschenes et al. (2012).[25] Additionally, in Table A2.5, we find that SO_2 emissions statistically significantly decreased by around 7.1%. However, the effect on CO_2 emissions is statistically insignificant. Although there are some reductions in SO_2 emissions, the magnitude is relatively economically small. Moreover, Curtis (2014) found that employment in the manufacturing sector decreased due to the NBP market. As local economic conditions also influence criminal activities, such an effect of the NBP on the labor force may threaten the validity of the exclusion restriction assumption. Deschenes et al. (2012), however, found that electricity prices were not affected by the NBP market. Considering these potentially confounding factors, we argue that the IV estimates should be treated with

[25] In Deschenes et al. (2012), they assigned emissions of zero to counties with no recorded emissions, while we do not include these counties. This is because NO_x can travel long distances. The NO_x concentrations in these counties are not necessary to be zero. Although the total reduction of NO_x emissions estimated in our study are higher than that in Deschenes et al. (2012), the reduction percentages are similar to each other.

caution.

First, the effects of NO_x emissions on violent crimes are shown in columns (1)-(4) in Table 2.11. The results indicate a evident association between NO_x emissions and violent crimes; the effects on rapes and robberies, for instance, are statistically significant at the 10% level. A 1,000-ton reduction in NO_x emissions lowers rape and robbery rates by 6.1% and 5.6%, respectively. Although the estimates on murders and assaults are not precise, the signs are positive and the magnitudes are not negligible. Columns (5)-(7) in Table 2.11 present the estimates on property criminal activities. The coefficients are smaller than those for violent crimes, and the sign for motor vehicle thefts is even negative. The findings therefore suggest that NO_x emissions affect violent criminal behaviors but not property crimes.

Next, we compare the IV estimates to the fixed-effect estimates, which are shown in Table 2.2. The fixed-effect estimates provide little evidence that NO_x emissions have a strong relationship with criminal activities. As we discussed before, this is possibly because the fixed-effect estimates may suffer from omitted variable and attenuation bias. As can be seen in IV estimates, the NO_x's effects on violent crimes become larger.

2.5.5 Sensitivity analysis

Falsification test. Based on the potential mechanisms that explain the relationships between air pollution and criminal activities, we expect that manslaughters should not be affected much by air pollution. This is because manslaughter is the killing of another person through gross negligence. Table 2.12 presents the impact of the NBP on manslaughters. The estimates in columns (1) through (3) indicate that the NBP's effect on the manslaughter rate is not statistically significantly different from zero, and the signs of the coefficients are even positive. These results provide a reassuring placebo test.

Alternative start year. In general, the triple-difference estimator requires two-period (two-year) observations before the policy's initiation, based on which we can check the parallel trend assumption. By adding extra period observations in advance of the policy's initiation, the coefficients are likely to be estimated more precisely. The main conclusions should not change, however, with the selection of pre-treatment sample periods. Figure A2.1 plots the coefficients of interest based on different sample periods. The figure shows that our main conclusions are not sensitive to the choice of sample period start year. Specifically, in the four distinct sample periods, the NBP's effects on rapes and robberies are significantly negative (at the 5% level); the coefficients for murders and assaults are either negative or close to zero; and the NBP's effects on property crimes are not statistically significant across different sample periods.

Adjacent states. In the main results, states adjacent to NBP states are excluded because the treatment status is unclear. As winds can blow air pollution far away, it is possible that air pollution concentrations in states adjacent to NBP states also decrease. To check whether our results are robust to this step, we first include these states as the control group. Table A2.6 shows the corresponding results. In each column, county-by-season, season-by-year, county-by-year fixed effects, and flexible weather controls are added in regressions. By comparing these estimates to our main results, we find that our conclusions remain almost unchanged. Next, we designate these states adjacent to NBP states as the treatment group. The corresponding results are presented in Table A2.7; again, coefficients change only slightly relative to our main results. To sum up, our main results are insensitive to such changes.

Alternative clusters. In the main specification, standard errors are clustered at the state-season level, allowing autocorrelations across year. We acknowledge, however, that there may exist arbitrary autocorrelation within states or state-years. To check the robustness of statistical inferences, we change the standard error clusters in regressions. Standard errors in Tables A2.2 and A2.3 are clustered by state-year and state level, respectively. To compare with the main results, we use the richest specification for each regression. We find that the magnitudes of the new clustered standard errors are similar to those in our main results. Therefore, our inferences are, in general, little affected.

2.6 Concluding remarks and implications

This paper examines the causal effects of air pollution on criminal activities, employing a well-known quasi-experiment—the NO_x Budget Trading Program, which has been documented to dramatically reduce NO_x emissions and ozone concentrations in participating states. Using a triple-difference method, we find that violent crimes in the participating states statistically significantly decreased. However, burglary, larceny, and motor vehicle theft rates were less affected by the NBP. Instrumental variable estimates suggest that NO_x emissions are positively correlated with violent criminal behaviors, indicating that lowering pollution emissions may play an important role in reducing violent crimes. In comparison, fixed-effect estimates show that the effects are negligible.

We end by using our estimates to conduct a back-of-the-envelope calculation, with a view to drawing implications from these results. McCollister et al. (2010) reported the potential social costs, both tangible and intangible, for each crime type in 2008 dollars. Specifically, the social costs for a case of murder, rape, robbery, and assault are \$9 million, \$240,776, \$42,310, and \$66,888, respectively.[26] According to our estimates, this cap-and-trade market decreased rapes and robberies in the Eastern U.S. by 386.4 and 2,148.6 cases per year, respectively.[27] In total, the NBP saved around \$184 million per year in societal costs. If we further take murders and assaults into account, total societal costs saved by the NBP reach about \$882 million.[28] Relative to the costs of the NBP (\$400-700 million per year as estimated by Deschenes et al. (2012)), these benefits were not negligible.

[26]The social costs for assaults are the mean of that for aggravated and simple assaults. Following Ranson (2014), we value the costs for simple assaults as 25% of that for aggravated assaults.

[27]Murders and assaults were reduced by 36.0 and 5,593.0 cases per year, respectively, although the estimated effects are not statistically significant.

[28]Estimated savings heavily depend on how to value the social cost of each crime type. The figure we calculate here, therefore, is merely a rough magnitude.

Tables and Figures of Chapter Two

Table 2.1: Descriptive statistics

Variables	(1) N	(2) Mean	(3) Std.Dev.	(4) Minimum	(5) Maximum
Population (1,000)	36,632	109.93	347.51	1.02	10,024.95
The NBP county	1,737	0.42	0.49	0.00	1.00
Actual offenses per 1,000 people					
Num. of murders	36,632	0.02	0.04	0.00	1.83
Num. of rapes	36,632	0.14	0.15	0.00	8.44
Num. of forcible rapes	36,632	0.13	0.14	0.00	8.37
Num. of attempted forcible rapes	36,632	0.01	0.03	0.00	1.08
Num. of robberies	36,632	0.23	0.36	0.00	4.67
Num. of firearm robberies	36,632	0.09	0.17	0.00	2.64
Num. of strong arm robberies	36,632	0.10	0.16	0.00	2.99
Num. of assaults	36,632	5.95	4.34	0.00	165.01
Num. of aggravated assaults	36,632	0.44	0.64	0.00	20.76
Num. of simple assaults	36,632	4.76	3.58	0.00	144.27
Num. of burglaries	36,632	3.29	2.24	0.00	69.43
Num. of forcible entries	36,632	1.97	1.65	0.00	54.10
Num. of unlawful entries	36,632	1.12	0.90	0.00	19.22
Num. of larcenies	36,632	9.50	6.06	0.00	173.98
Num. of motor vehicles thefts	36,632	0.97	1.01	0.00	44.04
Num. of auto thefts	36,632	0.66	0.75	0.00	26.15
Num. of trucks and buses thefts	36,632	0.16	0.25	0.00	13.76
Pollution emissions (1,000 tons)					
NO_x Emissions	10,089	3.18	4.87	0.00	58.79
SO_2 Emissions	10,089	7.58	13.36	0.00	127.11
CO_2 Emissions	10,089	1,798.52	2,275.16	0.00	14,046.22
Weather					
Average temperature (^{0}F)	36,632	58.75	15.04	20.01	92.77
Average precipitation (mm)	36,632	1.94	1.49	0.00	13.57
Average dew point (^{0}F)	36,632	45.60	14.99	0.00	75.88

Note: The crime-data sample includes 1,737 counties in 37 states. Crimes are totals per 1,000 people per county-season-year. Pollution emissions are mean values in each county-year-season cell. Winter emissions are multiplied by 5/7, so all values are summer-equivalent. Means are across counties (i.e., not weighted). The sample covers the period from 1998 through 2008.

Table 2.2: Impacts of NO_x emissions on violent and property criminal activities (fixed-effect estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
NO_x Emission (1,000 tons)	0.001	0.004	0.001	-0.000	-0.002	0.002*	0.002
	(0.005)	(0.004)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
Observations	23,544	23,544	23,544	23,544	23,544	23,544	23,544
R-squared	0.900	0.893	0.962	0.980	0.966	0.978	0.967
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of crimes per 1,000 people per county-season-year (in logarithm). Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by county-season.

Table 2.3: Impact of the NBP on murders

VARIABLES	(1) Murder	(2) Murder	(3) Murder
DDD	-0.022*	-0.020	-0.021
	(0.012)	(0.013)	(0.018)
Observations	36,632	36,632	36,632
R-squared	0.768	0.768	0.903
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is the number of murders per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.4: Impact of the NBP on rapes

	(1)	(2)	(3)
VARIABLES		**A. Rape**	
DDD	-0.026**	-0.035**	-0.038**
	(0.013)	(0.015)	(0.019)
Observations	36,632	36,632	36,632
R-squared	0.639	0.639	0.883
VARIABLES		**B. Forcible rape**	
DDD	-0.028**	-0.039***	-0.042**
	(0.013)	(0.014)	(0.019)
Observations	36,632	36,632	36,632
R-squared	0.638	0.638	0.881
VARIABLES		**C. Attempted forcible rape**	
DDD	-0.015	-0.020	-0.018
	(0.015)	(0.018)	(0.024)
Observations	36,632	36,632	36,632
R-squared	0.781	0.781	0.926
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in panels A, B, and C are the number of rapes, forcible rapes, and attempted forcible rapes per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.5: Impact of the NBP on robberies

	(1)	(2)	(3)
VARIABLES		**A. Robbery**	
DDD	-0.036***	-0.045***	-0.047**
	(0.012)	(0.014)	(0.020)
Observations	36,632	36,632	36,632
R-squared	0.871	0.871	0.957
VARIABLES		**B. Firearm robbery**	
DDD	-0.015	-0.025*	-0.021
	(0.012)	(0.013)	(0.018)
Observations	36,632	36,632	36,632
R-squared	0.846	0.846	0.946
VARIABLES		**C. Strongarm robbery**	
DDD	-0.050***	-0.057***	-0.061**
	(0.014)	(0.016)	(0.023)
Observations	36,632	36,632	36,632
R-squared	0.816	0.816	0.942
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in panels A, B, and C are the number of robberies, firearm robberies, and strongarm robberies per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.6: Impact of the NBP on assaults

	(1)	(2)	(3)
VARIABLES		**A. Assault**	
DDD	-0.001	-0.012	-0.012
	(0.007)	(0.008)	(0.010)
Observations	36,632	36,632	36,632
R-squared	0.833	0.833	0.978
VARIABLES		**B. Aggravated assault**	
DDD	0.000	-0.005	-0.005
	(0.011)	(0.013)	(0.015)
Observations	36,632	36,632	36,632
R-squared	0.660	0.660	0.933
VARIABLES		**C. Simple assault**	
DDD	-0.003	-0.012	-0.012
	(0.008)	(0.009)	(0.012)
Observations	36,632	36,632	36,632
R-squared	0.803	0.803	0.976
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in panels A, B, and C are the number of assaults, aggravated assaults, and simple assaults per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.7: Impact of the NBP on burglaries

	(1)	(2)	(3)
VARIABLES		**A. Burglary**	
DDD	0.001	-0.010	-0.009
	(0.007)	(0.008)	(0.010)
Observations	36,632	36,632	36,632
R-squared	0.826	0.826	0.961
VARIABLES		**B. Forcible entry**	
DDD	0.001	-0.012	-0.007
	(0.007)	(0.009)	(0.010)
Observations	36,632	36,632	36,632
R-squared	0.808	0.808	0.960
VARIABLES		**C. Unlawful entry**	
DDD	0.005	-0.004	-0.006
	(0.009)	(0.010)	(0.013)
Observations	36,632	36,632	36,632
R-squared	0.708	0.709	0.944
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in panels A, B, and C are the number of burglaries, forcible entries, and unlawful entries per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.8: Impact of the NBP on larcenies

VARIABLES	(1) Larceny	(2) Larceny	(3) Larceny
DDD	-0.007	-0.017**	-0.015
	(0.007)	(0.008)	(0.010)
Observations	36,632	36,632	36,632
R-squared	0.835	0.836	0.978
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is the number of larcenies per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.9: Impact of the NBP on motor vehicle thefts

	(1)	(2)	(3)
VARIABLES		**A. Motor vehicle theft**	
DDD	0.013	0.008	0.005
	(0.010)	(0.011)	(0.015)
Observations	36,632	36,632	36,632
R-squared	0.854	0.854	0.960
VARIABLES		**B. Auto theft**	
DDD	0.010	0.005	0.005
	(0.008)	(0.010)	(0.013)
Observations	36,632	36,632	36,632
R-squared	0.845	0.845	0.958
VARIABLES		**C. Trucks and buses theft**	
DDD	0.008	-0.002	-0.012
	(0.016)	(0.017)	(0.021)
Observations	36,632	36,632	36,632
R-squared	0.796	0.796	0.948
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in panels A, B, and C are the number of motor thefts, auto thefts, and trucks and buses thefts per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.10: Validity checks on the identification assumption for the triple-difference estimator

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
NBP	0.008	-0.016*	-0.002	0.005	-0.005	0.001	-0.004
	(0.008)	(0.009)	(0.008)	(0.005)	(0.008)	(0.005)	(0.008)
Observations	8,200	8,200	8,200	8,200	8,200	8,200	8,200
R-squared	0.219	0.239	0.263	0.293	0.265	0.423	0.256
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the differences of log of criminal activities per 1,000 people between summer and winter within a county-year cell. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 2.11: Impacts of NO_x emissions on violent and property criminal activities (IV estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
NO_x Emission (1,000 tons)	0.038	0.061*	0.056*	0.022	0.019	0.026	-0.001
	(0.028)	(0.033)	(0.029)	(0.018)	(0.015)	(0.017)	(0.022)
Sanderson-Windmeijer *F-stat*	24.37	24.37	24.37	24.37	24.37	24.37	24.37
Observations	23,544	23,544	23,544	23,544	23,544	23,544	23,544
R-squared	0.900	0.891	0.962	0.979	0.965	0.977	0.967
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

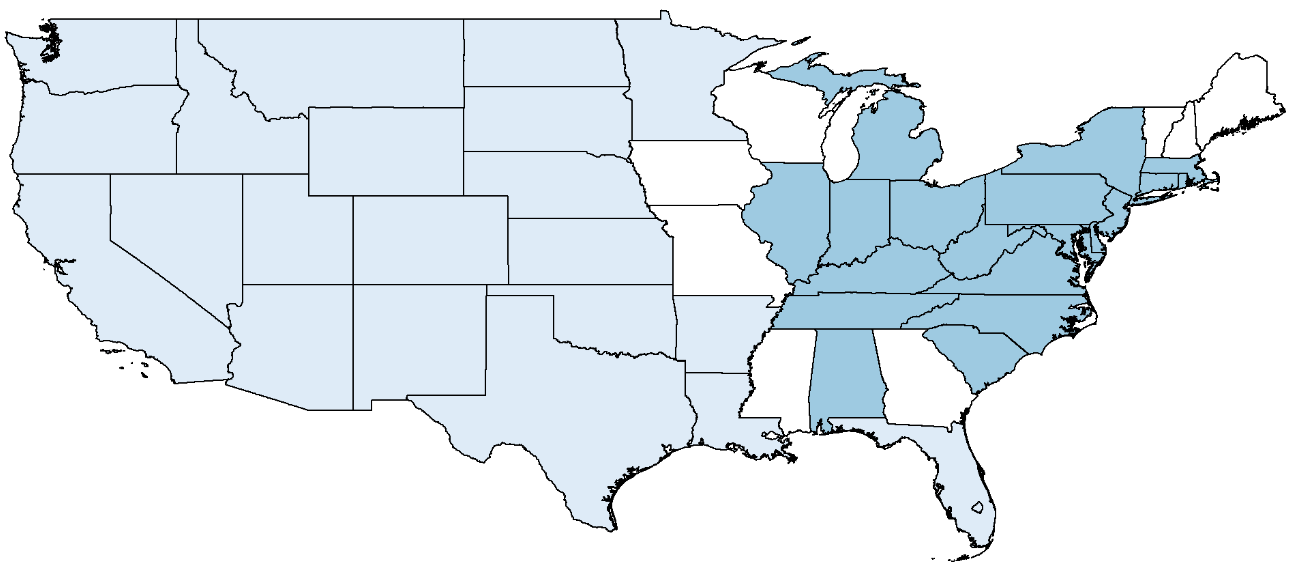
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of crimes per 1,000 people per county-season-year (in logarithm). NO_x emissions is the endogenous variable, which is instrumented by DDD. Regressions are two-stage least squares with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table 2.12: Impact of the NBP on manslaughters

VARIABLES	(1) Manslaughter	(2) Manslaughter	(3) Manslaughter
DDD	0.016	0.017	0.015
	(0.017)	(0.017)	(0.026)
Observations	36,632	36,632	36,632
R-squared	0.959	0.959	0.985
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variable is the number of manslaughters per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Figure 2.1: NBP regions



Note: Dark blue states are those participating in the NBP during the 2003-2008 period (the NBP states). Light blue states are not participating (non-NBP states). White states, which did not participate in the NBP but are adjacent to NBP states, are: Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, Vermont, and Wisconsin. We exclude these states in our analysis. Additionally, non-continental states (Alaska and Hawaii) and Puerto Rico are also not included. Alabama, Florida, and Illinois are also deleted because their crime data do not satisfy the requirements for the present analysis (see details in data section). As only a few counties in Michigan participated the NBP, we do not include it in the analysis.

Figure 2.2: Impacts of the NBP on violent and property crimes across years

Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.

Appendix of Chapter Two

Table A2.1: DD estimates (county-year level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
DD	-0.018	-0.030**	-0.037***	-0.009*	-0.006	-0.013*	0.002
	(0.016)	(0.012)	(0.013)	(0.005)	(0.007)	(0.008)	(0.008)
Observations	18,316	18,316	18,316	18,316	18,316	18,316	18,316
R-squared	0.903	0.883	0.957	0.978	0.961	0.978	0.960
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-specific Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of criminal activities per 1,000 people per county-year (in logarithm). **DD** is the difference-in-difference estimator, which equals to 1 for all counties belonging to NBP states in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year. Standard errors in parentheses, clustered by state.

Table A2.2: Sensitivity analysis (state-year clusters)

VARIABLES	(1) Murder	(2) Rape	(3) Robbery	(4) Assault	(5) Burglary	(6) Larceny	(7) Motor vehicle theft
DDD	-0.021	-0.038**	-0.047***	-0.012*	-0.009	-0.015**	0.005
	(0.020)	(0.016)	(0.015)	(0.007)	(0.009)	(0.007)	(0.011)
Observations	36,632	36,632	36,632	36,632	36,632	36,632	36,632
R-squared	0.903	0.883	0.957	0.978	0.961	0.978	0.960
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-year.

Table A2.3: Sensitivity analysis (state clusters)

VARIABLES	(1) Murder	(2) Rape	(3) Robbery	(4) Assault	(5) Burglary	(6) Larceny	(7) Motor vehicle theft
DDD	-0.021	-0.038*	-0.047**	-0.012	-0.009	-0.015	0.005
	(0.018)	(0.019)	(0.020)	(0.010)	(0.010)	(0.010)	(0.015)
Observations	36,632	36,632	36,632	36,632	36,632	36,632	36,632
R-squared	0.903	0.883	0.957	0.978	0.961	0.978	0.960
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state.

Table A2.4: Impact of the NBP on NO_x emissions

VARIABLES	(1) NO_x	(2) NO_x	(3) NO_x
DDD	-1.687***	-1.692***	-1.703***
	(0.294)	(0.287)	(0.382)
Pre-2003 mean	5.144	5.144	5.144
Observations	10,089	10,089	10,089
R-squared	0.879	0.880	0.971
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county-year-season. Winter emissions are multiplied by 5/7, so all values are summer-equivalent. Response variable measured in thousands of tons. Mean represents 1998-2002 summer in NBP areas. **DDD** is the triple difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Standard errors in parentheses, clustered by county-season.

Table A2.5: Impact of the NBP on SO_2 and CO_2 emissions

VARIABLES	(1) SO_2	(2) SO_2	(3) SO_2	(4) CO_2	(5) CO_2	(6) CO_2
DDD	-1.019***	-0.964***	-1.019***	-36.659	-41.757*	-47.461
	(0.235)	(0.245)	(0.307)	(26.614)	(22.292)	(29.993)
Pre-2003 mean	14.411	14.411	14.411	2136.117	2136.117	2136.117
Observations	10,089	10,089	10,089	10,089	10,089	10,089
R-squared	0.920	0.920	0.992	0.977	0.978	0.994
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No	Yes	Yes	No
County-by-Year FE	No	No	Yes	No	No	Yes
Flexible Weather Controls	No	Yes	Yes	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county-year-season. Winter emissions are multiplied by 5/7, so all values are summer-equivalent. Response variable measured in thousands of tons. Mean represents 1998-2002 summer in NBP areas. **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Standard errors in parentheses, clustered by county-season.

Table A2.6: Sensitivity analysis (including adjacent states as the control group)

VARIABLES	(1) Murder	(2) Rape	(3) Robbery	(4) Assault	(5) Burglary	(6) Larceny	(7) Motor vehicle theft
DDD	-0.004	-0.038**	-0.032*	-0.009	-0.005	-0.013	0.012
	(0.017)	(0.016)	(0.017)	(0.008)	(0.008)	(0.008)	(0.013)
Observations	47,512	47,512	47,512	47,512	47,512	47,512	47,512
R-squared	0.904	0.882	0.958	0.979	0.960	0.978	0.959
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table A2.7: Sensitivity analysis (including adjacent states as the treatment group)

VARIABLES	(1) Murder	(2) Rape	(3) Robbery	(4) Assault	(5) Burglary	(6) Larceny	(7) Motor vehicle theft
DDD	-0.019	-0.033*	-0.043**	-0.009	-0.005	-0.009	0.009
	(0.017)	(0.017)	(0.018)	(0.009)	(0.009)	(0.010)	(0.014)
Observations	47,512	47,512	47,512	47,512	47,512	47,512	47,512
R-squared	0.904	0.882	0.958	0.979	0.960	0.978	0.959
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state-season.

Table A2.8: Validity checks on the identification assumption for the triple-difference estimator (1999-2002)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
NBP	0.023**	0.009	-0.005	0.005	0.003	0.005	0.003
	(0.011)	(0.012)	(0.012)	(0.007)	(0.009)	(0.005)	(0.009)
Observations	6,615	6,615	6,615	6,615	6,615	6,615	6,615
R-squared	0.256	0.293	0.317	0.354	0.321	0.469	0.307
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the differences of log of criminal activities per 1,000 people between summer and winter within a county-year cell. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table A2.9: Validity checks on the identification assumption for the triple-difference estimator (2000-2002)

VARIABLES	(1) Murder	(2) Rape	(3) Robbery	(4) Assault	(5) Burglary	(6) Larceny	(7) Motor vehicle theft
NBP	0.036	0.012	-0.009	-0.015	-0.008	-0.010	-0.024
	(0.023)	(0.016)	(0.023)	(0.012)	(0.018)	(0.010)	(0.015)
Observations	5,053	5,053	5,053	5,053	5,053	5,053	5,053
R-squared	0.347	0.364	0.388	0.431	0.400	0.522	0.389
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

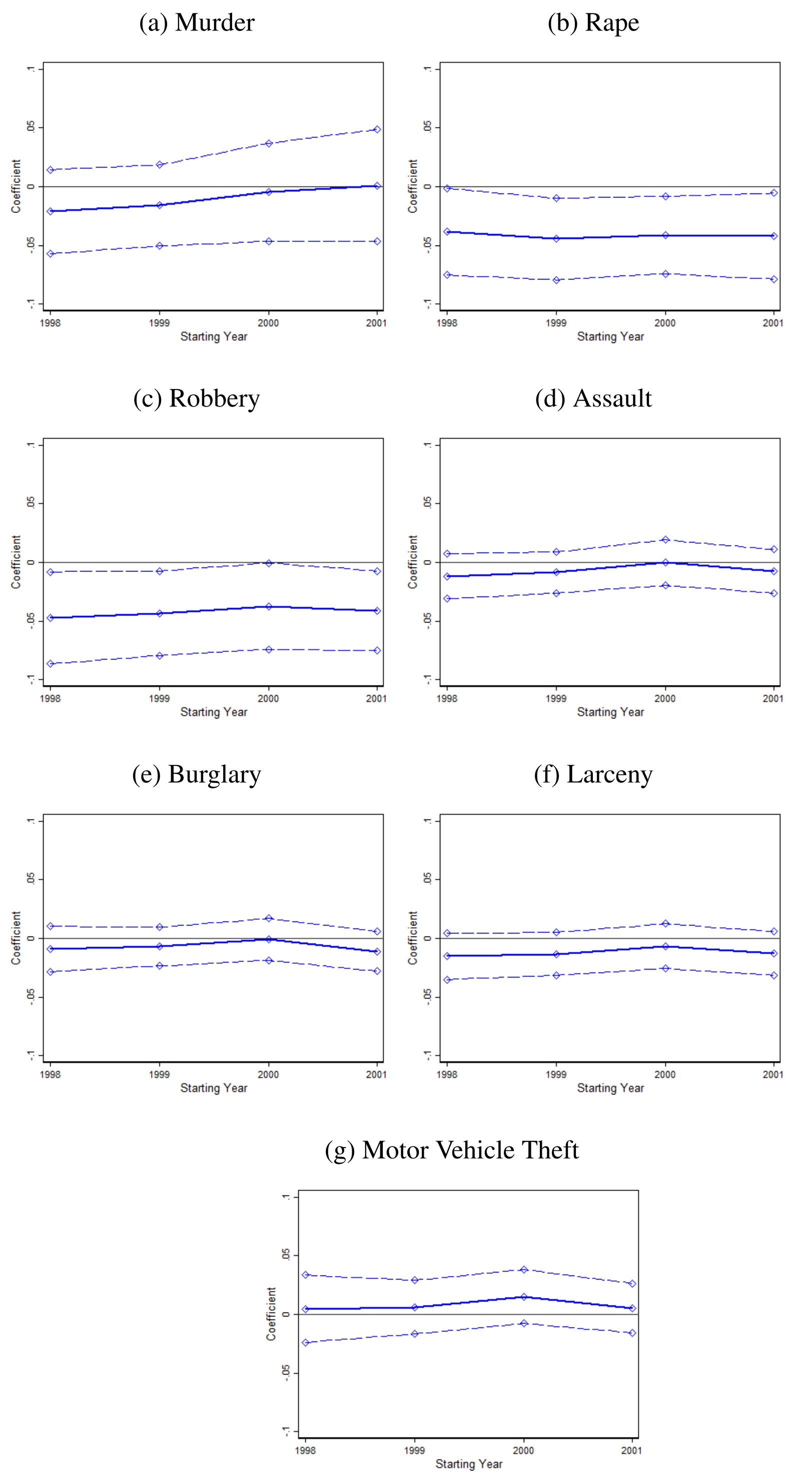
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the differences of log of criminal activities per 1,000 people between summer and winter within a county-year cell. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table A2.10: Validity checks on the identification assumption for the triple-difference estimator (2001-2002)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
NBP	0.048	0.032	-0.052	-0.008	0.046*	-0.017	0.017
	(0.046)	(0.045)	(0.052)	(0.018)	(0.026)	(0.015)	(0.026)
Observations	3,369	3,369	3,369	3,369	3,369	3,369	3,369
R-squared	0.515	0.529	0.544	0.603	0.560	0.670	0.557
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the differences of log of criminal activities per 1,000 people between summer and winter within a county-year cell. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Figure A2.1: Estimates based on alternative sample periods



Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.

Chapter 3

Environmental Regulation as a Double-edged Sword for Housing Markets—Evidence from the NO_x Budget Trading Program

3.1 Introduction

Previous studies have shown that air quality regulations generated large health benefits (Chay and Greenstone 2003; Currie and Neidell 2005; Deschenes et al. 2012; Schlenker and Walker 2015). Hedonic theory predicts that house prices should shift up as air pollution levels decrease. A large empirical evidence has documented the relationships between air quality improvement and housing values (Bayer et al. 2009; Chay and Greenstone 2005; Currie et al. 2015; Kim et al. 2003; Luechinger 2009; Zheng et al. 2010). However, environmental policies may also add cost to labor markets in regulated regions (Curtis 2014; Greenstone 2002; Greenstone et al. 2012; Kahn and Mansur 2013; Walker 2011; Walker 2013). To take a concrete example, Curtis (2014) found that a well-known cap-and-trade market, the NO_x Budget Trading Program (NBP), resulted in a statistically significant decline in manufacturing employment in participating states. The harmful side effect of this policy on labor markets may weaken the housing demand and further dampening house price growth, especially for manufacturing dominated areas. Therefore, the answer to the question that how environmental policies influence housing markets can be ambiguous. It should depend on to what extent air quality is improved and how local businesses are impacted.

Exploiting a quasi-experiment—the NBP, this article examines how this cap-and-trade system influenced housing markets in the participating regions. The NBP was designed to reduce ozone concentrations in Eastern U.S. by restricting nitrogen oxides (NO_x) emissions.[1] It was initiated in 2003 and ended in 2008. Nineteen states, together with Washington, DC, were included in this program (see Figure 3.1). Deschenes et al. (2012) found that the NBP dramatically reduced NO_x emissions and thus ozone pollution in the NBP states. As a result of air quality improvement, their study also showed that health benefits, in terms of medication expenditures and mortality rates, were non-negligible.

[1] NO_x is a major precursor of ozone formation.

However, the NBP also add substantial costs to manufacturing plants, leading to lower hiring rates and earning reductions, especially for young workers aged between 22 and 34 (Curtis 2014). As shown in Figure 3.2, this age group represents the main force that drives the housing demand. Therefore, we hypothesis that house prices in NBP regions with high manufacturing intensity were negatively affected by the NBP, resulted from its impacts on labor markets, but housing markets in low-manufacturing-intensity area may benefit from air pollution abatement.

Using the zip code-year level house price data obtained from Zillow.com, we exploit time and geographic variations to perform a difference-in-differences analysis that estimates the overall effect of the NBP on housing markets in the participating states. Figure 3.3 provides us a raw picture that how the NBP affected housing markets in the NBP states. As can be seen, before 2003 the NBP and non-NBP states shared a similar trend in house prices. After the market's operation, we find that compared to the housing markets in the non-NBP states, those in the NBP states did not gain much as a result of the environmental regulation. After accounting for zip code and year fixed effects and zip code-specific trends, our regression results indicate that the overall impact of the NBP is not statistically significantly different from zero.

To further examine the effects of potentially heterogeneous treatments, we take advantage of the heterogeneity in local business patterns for different counties. Specifically, we derive the industry employment data from the County Business Patterns (CBP) and construct a measurement of manufacturing intensity for each county, i.e., the ratio between manufacturing employment and total labor force in 1998.[2] Our results indicate that for areas without manufacturing employment in 1998, house prices rose 12.69% as a result of air quality improvement. This finding is consistent with the hedonic theory. More importantly, we find that a 1% increase in manufacturing intensity in 1998 statistically significantly reduced house price increase by 0.046%. For perspective, house prices in an area where manufacturing intensity in 1998 was 40% decreased by 4.63%. In contrast, house prices in an area in an area where manufacturing intensity in 1998 was 5% shifted up by 4.17%. These estimates imply that the negative impacts of the NBP on labor markets further dampening house price growth in high manufacturing intensity regions.

Our study makes two important contributions to the literature. First, to our best knowledge, the current study is the first to present the impacts of a large-scale cap-and-trade market on housing markets. Compared to the command and control style regulations, e.g., National Ambient Air Quality Standards, cap-and-trade systems have their advantages in terms of efficiency. Although emission markets provide a market-based solution to abate air pollution, they may also trigger adverse effects (Curtis 2014). Given the ongoing academic and policy debates on energy sector regulations, comprehensively understanding the impacts of emission markets is of high relevance. Second, this study provides evidence that environmental policy could also negatively impact housing markets due to its adverse effects on labor markets. Such harmful side effect on housing markets of environmental regulations has not been documented in the previous studies. As a result of

[2]This ratio may be affected by the NBP after the market's initiation. Thus we choose the ratio in 1998 as an exogenous measure of manufacturing intensity.

the emission market, house prices in the NBP areas with low and high manufacturing intensity increased and decreased, respectively. People who owned houses in low manufacturing intensity areas enjoyed an increase in their wealth, but their counterfactuals in high manufacturing intensity regions suffered a loss. In other words, the NBP generated an unintentionally impact on wealth redistribution.

The rest of the paper is organized as follows: the second section provides a qualitative analysis on the NBP impacts on housing markets; Section 3 summarizes our data sources and shows the descriptive analysis; Section 4 introduces our empirical framework; main findings and sensitivity analysis are presented in Section 5; we conclude in Section 6.

3.2 Qualitative analysis

The NBP was a cap-and-trade system which limited the NO_x emissions in Eastern states. As NO_x is a key ingredient of ozone formation, the target of the NBP was to reduce ozone air pollution.[3] The program formally started in 2003, including eight Northeast states together with Washington, DC.[4] In 2004, another 11 states joined the NBP.[5] As ozone concentrations are normally high in the summer but low in the winter, the cap-

[3]Details about the NBP market have been documented by Fowlie (2010); Deschenes et al. (2012); Curtis (2014).

[4]The eight states are Connecticut, Delaware, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and Rhode Island, respectively.

[5]The 11 states are Alabama, Illinois, Indiana, Kentucky, Michigan, North Carolina, Ohio, South Carolina, Tennessee, Virginia, and West Virginia. Among them, only a few of counties in Alabama and Michigan entered the market. Additionally, part of Missouri participated the NBP in 2007. In our analysis, we do not exploit the seasonal variation of house prices, because the variation is relatively small as one would expect with a long-term investment.

and-trade system only operated from May to September.[6] According to USEPA (2009), 2,500 electricity generating units and industrial boilers were enrolled in this cap-and-trade market. Among them, 700 coal-fired plants occupied around 95% NO_x emissions in the market.

Deschenes et al. (2012) found that the NBP dramatically reduced NO_x emissions by around 34-38%. Correspondingly, the ozone pollution concentrations in the NBP states decreased by about 7%. As a result of air quality improvement, pharmaceutical expenditures and mortality rates declined as well. Based on their estimates, the health benefits from the NBP were substantial. Hedonic theory predicts that with other factors constant, house prices will increase as air quality becomes better (Chay and Greenstone 2005). Specifically, in the Rosen (1974) model, house prices can be denoted as a function of the houses' characteristics, e.g., number of units, built time, air quality, and so forth:

$$H = H(a_1, a_2, ..., a_n). \tag{3.1}$$

By taking the partial derivative of $H(\cdot)$ with regard to the air quality of where houses are located, we can derive the marginal implicit price of air quality. As the NBP improved the air quality in Eastern states, the house prices in these regions are expected to rise, holding other factors constant.

In addition to health effects, the NBP also added substantial costs to regulated plants.

[6] One exception is in 2004 the NBP operated from June to September.

To comply with the NBP regulations, regulated plants have several strategies. First, instead of making any changes to production processes, plants may simply purchase permits to offset emissions exceeding their allocation. USEPA (2009) showed that around 30% regulated firms adopted this strategy. Second, the coal-fired plants may switch to cleaner energy sources (e.g., natural gas). Fowlie (2010) documented that few plants changes their energy sources, because cleaner energy sources are much more costly than coal. The third one is to adopt emission control technology. An efficient NO_x control technology, called selective catalytic reduction (SCR), can reduce up to 90% NO_x emissions. But the average cost is about 40 million dollars (Linn 2008). The last strategy is to reduce production during the regulated seasons.

Although a variety of strategies are available for regulated plants, they all shift up the production costs. A number of studies has estimated the cost of the NBP to the regulated plants (e.g., Deschenes et al. 2012; Fowlie et al. 2012; Linn 2010; Shapiro and Walker 2015). For instance, Deschenes et al. (2012) estimated the annual cost of the NBP to be 400-700 million dollars. Such sizable cost forced the plants to employ less labor. Using a triple-difference strategy, Curtis (2014) found that the NBP resulted in a 1.3% drop in the manufacturing employment and new hire earnings fell by around 4%. Particularly, as shown in Figure 3.4, workers aged between 22-34 were affected most.[7] Based on the data from the American Housing Survey (the national survey) in 1997-2007, we depict the age distribution of home-buyers for both first-time and repeat buyers in Figure 3.2. Panel (a)

[7] Figure 3.4 is quoted from Curtis (2014), which displays event-time coefficient estimates for five age groups based on Equation (3) in Curtis (2014).

describes the age distribution of first-time buyers. Particularly, the median age is around 31, and the age group between 22-34 occupies nearly 60%. For repeat buyers shown in Panel (b), around one fourth are aged between 22-34. Therefore, negative impacts of the NBP on the labor market are very likely to further induce the demand decline for housing markets in the NBP states (Roback 1982).

Based on qualitative predictions that we discussed above, the overall effect of the NBP on housing markets is ambiguous. On one hand, as the cap-and-trade market reduced pollution emissions dramatically, hedonic theory suggests that holding other factors constant, house prices should shift up. On the other hand, the environmental regulation had negative impacts on manufacturing employment outcomes, which may induce the demand decline for housing markets. Although predicting the overall effect of the NBP on housing markets is challenging, it may be plausible to hypothesize that the labor market effects dominate in high manufacturing intensity regions, whereas in low manufacturing intensity regions health effects are critical. The main target of the current study is to provide evidence of which housing markets were affected and how they were affected.

3.3 Data and descriptive analysis

3.3.1 Data sources

House prices. Our primary data on house prices at the zip code-month level come from Zillow.com. Instead of the median sale price, the Zillow home value data measure the value of all houses, no matter whether the homes are sold in a given month. It covers not only single-family homes, but also condominium and cooperative homes. The data are available for more than 10,000 zip code areas, representing more than 95% of the total housing stock by value.[8] Our sample period starts from 1998 and ends in 2008 when the NBP was replaced by another program called the Clean Air Interstate Rule (CAIR). As documented by Mian and Sufi (2009), Zillow data have good accuracy. Specifically, they found that the correlation between the Zillow Home Value Index (ZHVI) and Fiserv Case Shiller Weiss (FCSW) index reaches 0.91.[9] As Zillow data are available for much more zip codes areas than FCSW data, we mainly use the former in the present study. Besides the ZHVI, Zillow also provides the median home value per square feet and price indices across house types, including single-family, condominium and cooperative homes.

County characteristics. County-year level employment data are obtained from the County Business Patterns (CBP) produced by Census Bureau. The CBP provides annual information on economics data by industry in each county, including the number of establishments, employment, and payroll. As our qualitative analysis demonstrates, the impacts of the NBP on house prices may vary by the local manufacturing intensity. We employ this data to create a measurement of manufacturing intensity—the ratio between manufacturing employment and total labor force in one county. As the ratio may be affected by the NBP after the market's initiation, we select the ratio in 1998 as an exoge-

[8] For details, please refer to http://files.zillowstatic.com/research/public/Zillow%20Real%20Estate%20Research%20-%20Why%20We%27re%20Different.pdf.

[9] Guerrieri et al. (2013) provided a detailed comparison between the ZHVI and FCSW index.

nous measure of manufacturing intensity for each county. Additionally, we also generate measurements of other industries' intensity, including agriculture, service, and others.[10]

The main source of variation in the manufacturing intensity is cross-county differences. The manufacturing intensity may be correlated with other county characteristics which are also determinants of house prices. We obtain the set of potential determinants, including age, educational attainment, and ethnicity, from the Census 2000. As shown in Table A3.1, the manufacturing intensity is negatively associated with the median age and the percentage of adults with a bachelor's or college's degree, but positively correlated with the percentage of the black and adults with a high school diploma.

In empirical analysis, we do not include non-continental states—i.e., Alaska and Hawaii—and Puerto Rico. Moreover, as Deschenes et al. (2012) argued, states adjacent to the NBP regions may also benefit from pollution reduction. This is because NO_x can be transported downwind to a long distance (Streets et al. 2001). These states—Arkansas, Florida, Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, Vermont, and Wisconsin—are also excluded.[11] Moreover, only limited counties in Michigan participated the NBP, and we do not include them. Sample statistics are summarized in Table 3.1. The final sample contains 8,275 zip code areas in 36 states.

[10]Others include mining, utilities, and construction industry.

[11]In sensitivity analysis, we include these states as the control group. Our results remain stable (see Table A3.2).

3.3.2 Descriptive analysis

To motivate our empirical analysis, we start by documenting the relationship between annual house price growth rate and local manufacturing intensity in 1998. Panel (a) in Figure 3.5 displays this relationship for both the NBP and non-NBP regions before the market's operation. The linear fitted lines indicate that the larger the percentage of manufacturing employment in 1998, the slower house prices grow. On top of that, it is worth noting that the two linear fitted lines are parallel to each other, suggesting that before 2003 both the NBP and non-NBP regions share the similar relationship between house price growth rate and manufacturing intensity. To compare, Panel (b) in Figure 3.5 plots the association after the market's initiation. Interestingly, the two linear fitted lines are no longer parallel to each other. Particularly, the slope for the NBP regions is steeper than that for the non-NBP regions, indicating that after 2003 the gaps in house price growth rate between high- and low-manufacturing-intensity areas significantly widen in the NBP states, relative to the non-NBP states.

Figure 3.6 provides another set of comparisons. In Panel (a), we contrast the same relationship as in Figure 3.5 between before and after 2003 in the non-NBP states. The two linear fitted lines demonstrate that the differences in house price growth rate between high- and low-manufacturing-intensity areas remain stable in the non-NBP regions. But as shown in Panel (b), the slope of the linear fitted line for after 2003 is relatively steeper than that of for before 2003. The comparison in Figure 3.6 provides the similar implication as in Figure 3.5, i.e., the house price growth rate in the high-manufacturing regions of the NBP states were dampened after the NBP's initiation, relative to that in the low-manufacturing areas.

We may interpret the patterns displayed in the two sets of comparisons as the causal effect of the emission market-induced negative impacts on the manufacturing labor market. Although air quality improvement is supposed to induce house price increase, the adverse effects of the NBP on the manufacturing employment may trigger the demand decline for housing markets in NBP states. But we admit that our interpretation of the patterns faces two major challenges. First, as we documented above, the source of variation in the manufacturing intensity is cross-county differences. High- and low-manufacturing regions may differ in other unobserved dimensions which may also explain the differential cross-county growth in house prices. Second, there may exist other policies which were conducted after 2003 and disproportionately affected housing markets in the NBP regions directly or indirectly. For instance, the EPA released a more restrictive ozone non-attainment standards in 2004.[12] The two concerns further motive our econometric framework.

[12]The details are discussed in the following section.

3.4 Empirical strategy

To examine the overall effects of the emission market on house prices, we adopt a difference-in-differences approach. This environmental regulation primarily provides us two dimensions of variations in house prices, i.e., before versus after the program's operation and participating versus non-participating states. Exploiting the variations, we estimate the following specification:

$$Y_{it} = \alpha(After_{it} \times NBP_i) + \mu_i + \lambda_t + \mu_i * t + \mu_i * t^2 + \epsilon_{it}. \tag{3.2}$$

where the dependent variable is the median home value per square feet in zip code i in year t. $After_{it}$ is a dummy variable, which equals to one after the market's initiation. The variable NBP_i indicates all areas that participated the NBP. The interaction term, $After_{it} \times NBP_i$, is designed to estimate the average effect of the emission market on house prices in the NBP areas. Zip code fixed effects, μ_i, are included to govern any time-invariant zip code level factors. λ_t represents the year fixed effects, capturing common shocks over years. The vectors of the zip-code-specific linear and quadratic time trends, $\mu_i * t$ and $\mu_i * t^2$, are further added, partialling out the nonlinear changes in the determinants of house prices. ϵ_{it} represents an idiosyncratic random error term. To adjust for potential temporal and spatial autocorrelations, standard errors are clustered at the state level.

As our qualitative analysis suggests, the sign of β is ambiguous. Specifically, it may be positive because air pollution abatement may induce a housing market boom. However, the negative effects of the NBP on workers in the manufacturing sector may dampen house price growth, especially for manufacturing dominated areas. To further explore how housing markets in areas with different business patterns were impacted by the cap-and-trade system, we take advantage of the manufacturing intensity heterogeneity across regions. To achieve the goal, we employ the following model:

$$Y_{it} = \beta(After_{it} \times NBP_i \times Manuf_c) + \gamma(After_{it} \times NBP_i) + \delta(After_{it} \times Manuf_c)$$
$$+ \mu_i + \lambda_t + \mu_i * t + \mu_i * t^2 + \varepsilon_{it}. \quad (3.3)$$

where $Manuf_c$ denotes the logged ratio between manufacturing employment and total labor force in county c in 1998.[13] The variable of interest is the three-way interaction, $After_{it} \times NBP_i \times Manuf_c$, capturing effects of the NBP on house prices in areas with different manufacturing intensities. Our main hypothesis is that the higher the manufacturing intensity, the more likely local economic activities and house prices were negatively affected by the NBP. In other words, we expect the coefficient β to be statistically significantly negative. Additionally, the coefficient γ measures the effect of the NBP on house prices in areas with no manufacturing employment in 1998, i.e., $Manuf_c = 0$. These districts are supposed to enjoy an improvement in air quality, and their economic activities should be affected less. Based on the hedonic model, we predict that the sign of γ should

[13] One may concern the particularity of the measurement in 1998. In robustness checks, we replace it with the average logged ratio between 1998 to 2002. As shown in Table A3.3, our main conclusions remain stable. Additionally, there are a few counties with no manufacturing intensity (less tha 1%). To account for that, we use a transformation with a logarithm of manufacturing employment percentage plus one.

be positive.

In Equation (2), the variation of the difference-in-differences estimator is at the state-year level. In contrast, the variation of the three-way interaction in Equation (3) stems from the county-year level, enabling us to control for nonlinear changes in house prices for each state more flexibly. As a robustness check, we replace the zip-code-specific linear and quadratic time trends with state-year fixed effects in Equation (3). During our sample period, the U.S. enacted several policies that likely created nonlinear state-specific differences over time, e.g., American Homeownership and Economic Opportunity Act of 2000 and American Dream Down-payment Initiative (2003). However, one limitation about this model is that the two way interaction, $After \times NBP$, will be absorbed by state-year fixed effects. Based on this model, we are unable to derive the magnitude of the NBP effect on housing markets for areas with different manufacturing intensities. Therefore, we use Equation (3) as our main specification, and employ the model with state-year fixed effects to examine the robustness of the coefficient on $After \times NBP \times Manuf$.

3.4.1 Identification assumption

The validity of this difference-in-differences estimator requires that house prices in the NBP and non-NBP regions share the parallel trend before the market's initiation. Figure 3.3 presents the median price per square feet in both the NBP and non-NBP regions during our sample period. It is evident that before the market's operation the trends of house prices in the treatment and control group are parallel to each other. Next, we employ a event-study framework to validate the parallel trend assumption, i.e., estimating the effect of the NBP for each year. The model is as follow:

$$Y_{it} = \sum_{t=1998}^{2008} \zeta_t (NBP_i \times Manuf_c) + \gamma(After_{it} \times NBP_i) + \delta(After_{it} \times Manuf_c)$$

$$\mu_i + \lambda_t + \epsilon_{it}. \quad (3.4)$$

In practice, we set 2002 as the reference group, i.e., $\zeta_{2002} = 0$. If coefficients for 1998 through 2001 are not statistically significantly different from zero, this may imply that there is no evidence of clear differences in the trend of house prices between the NBP and non-NBP states before 2003. Besides checking the common trend assumption, this method enables us to estimate the policy effect in each year after the market's initiation.

3.5 Main results

In this section, we begin by reporting the overall effect of the NBP on housing markets in the participating states. Then we present the NBP effects across counties with different manufacturing intensities, which are the central findings of this paper. Next section discusses the possible mechanisms. In the last part, a series of sensitivity analysis are conducted.

3.5.1 Overall effect of the NBP

Before the regression analysis, Figure 3.3 presents a visual depiction of how house prices in the participating states were affected by the cap-and-trade market. Before the market's operation, the trends of house prices in the NBP and non-NBP states are relatively parallel to each other. However, after 2003 house prices in the NBP regions seem to grow slower than that in the non-NBP regions, indicating that the housing markets in the NBP regions did not benefit much from the pollution abatement program.

Table 3.2 reports the statistical estimates of the overall effect of the NBP on median price per square feet. In column (1), we control the zip code and year fixed effects, and the coefficient is negative but not statistically significantly different from zero. Then in columns (2) and (3), we gradually add the zip code linear and quadratic trends to regressions. As can be seen, the coefficients becomes even closer to zero, and the confidence intervals are quite large. To sum up, both Figure 3.3 and Table 3.2 suggest that compared to the housing markets in the non-NBP states, those in the NBP states did not gain much due to the emission market.

3.5.2 Heterogeneous effects by manufacturing intensity

As we discussed above, house prices in the NBP regions with high manufacturing intensity are likely to be negatively affected as a result of the effects of the emission market on employment in the manufacturing sector. But house prices in low manufacturing intensity areas in the NBP states may shift up due to the air quality improvement. Table 3.3 presents estimates of several versions of Equation (3). Column (1) is our most parsimonious specification, which only includes two variables of interest, zip code, and year fixed effects. The sign of the coefficient on $After \times NBP$ indicates that the NBP effect on house prices in the NBP areas with no manufacturing employment in 1998 was positive. On top of that, the coefficient on $After \times NBP \times Manuf$ is statistically significantly negative, suggesting that the higher the manufacturing intensity, the more house prices were negatively affected by the NBP. These results seem to be consistent with our main hypothesis. To partial out the potentially nonlinear changes in the determinants of house prices, we further add the zip code linear and quadratic trends in columns (2) and (3), respectively. Compared to those in column (1), coefficients in columns (2) and (3) are smaller but remain statistically significant at the conventional level. The estimates in column (3) imply that house prices in NBP areas with no manufacturing employment in 1998 increased by 12.69%, a sizeable positive effect. More importantly, the coefficient on the three-way interaction indicates that a 1% increase in manufacturing intensity in 1998 reduced house price increase by 0.046%.

During our sample period, several federal housing policies may generate nonlinear state-specific differences in house prices over time. To more flexibly control policy shocks, we replace the zip code-specific linear and quadratic trends in Equation (3) with state-year fixed effects. Column (4) in Table 3.3 reports the estimate. Compared to that in column (3), the absolute magnitude of the three-way interaction term increases only slightly, from -0.0458 to -0.0396. And it is statistically significant at 6% level. Although the non-parametric model allows for a high degree of flexibility, the two way interaction, $After \times NBP$, is absorbed by state-year fixed effects. As discussed in the previous section, based on this model we are unable to obtain the magnitude of the NBP effect on housing markets for areas with different manufacturing intensities.

Next, we take concrete examples to interpret the estimates in column (3) in Table 3.3. For places with 40%, 30%, and 20% manufacturing intensity in 1998, the NBP decreased their house prices by 4.63%, 3.35% and 1.56%, respectively.[14] For districts with 10% and 5% manufacturing intensity in 1998, their house prices shifted up by 1.40% and 4.17%, respectively. These calculations assume a specific functional form of the effect in manufacturing intensity. To relax the functional form assumption, Table 3.4 presents non-parametric estimates. Specifically, we examine the NBP effect on regions with manufacturing intensity in 1998 less than 5%, 5-10%, 10-15%, 15-40%, 40-45%, 45-50%, and more than 50%, respectively. In column (1), we control zip code and year fixed effects and zip code linear trends. House prices in areas with the lowest and highest manufacturing intensity were statistically significantly affected. Additionally, the non-parametric estimates display a monotonic pattern. Specifically, the NBP effects on house prices for these seven groups are 6.19%, 3.02%, 3.73%, -1.78%, -2.48%, -3.19%, and -5.18%, respectively. In column (2), by adding zip code quadratic trends, we find that the monotonic pattern is robust.

[14]The figures are computed as follow: $4.63\% = (0.1238 + -0.0458 \times log(40+1)) \times 100\%$; $3.35\% = (0.1238 + -0.0458 \times log(30+1)) \times 100\%$; $1.56\% = (0.1238 + -0.0458 \times log(20+1)) \times 100\%$.

Identification assumption test. We adopt a event-study framework to examine the presence of trends in advance of market's operation. Using Equation (4), we can estimate the NBP effect on house prices in each year. The estimates are plotted in Figure 3.7. We set 2002 as the reference group, i.e., the coefficient on 2002 is zero,. As can be seen, all the coefficients in the 1998-2001 period are closed to zero and are not statistically significant at the traditional level. This evidence indicates the presence of only a slight pre-existing trend between the NBP and non-NBP states. Additionally, Figure 3.7 also vividly presents the NBP effect on house prices after 2003. Noticeably, after the market began to operate, the coefficients are all negative, varying between 0 and -0.05. Their absolute magnitudes gradually become larger from 2003 to 2007. This pattern indicates that our estimates in Table 3.3 are not driven by effects that come from some certain year. Furthermore, this pattern is similar to that shown in Curtis (2014). As shown in Figure 3.4, the magnitude of the NBP effects on labor markets became larger after 2003 gradually, and slightly decreased after 2006. The consistent patterns are reassuring for us to argue that the negative effects on housing markets come from the NBP effects on labor markets.

3.5.3 Mechanisms

Pollution emissions. One may concern that the heterogeneous effects of the NBP on house prices result from the heterogeneous effects on air pollution abatement. For perspective, pollution emissions in low manufacturing intensity areas may be reduced much more than those in high manufacturing intensity areas. Additionally, pollution emissions in high manufacturing intensity areas may somehow go up after the emission market's initiation, which can explain the negative impacts of the NBP on house prices in those areas. Table A3.4 tests this story. In column (1), the coefficient indicates that the NO_x emissions in the NBP operating season (summertime) in the NBP states decreased by around 17.34%. The estimates in column (2) suggest that the NBP effect on NO_x emissions do not vary by areas with different manufacturing intensities. In other words, NO_x emissions were consistently reduced in the NBP states. Therefore, our findings cannot be explained solely by air pollution abatement.

Non-manufacturing industries. As the NBP mainly decreased employment and earnings in the manufacturing sector, house prices should not be affected by the interactions between the NBP and other industry intensities. Columns (1) through (3) in Table 3.5 examine whether the proportion of agricultural, service, and other industry employment in 1998 plays an important role in determining house prices.[15] As can be seen, the three-way interactions in these columns are far from statistically significant at the conventional level. In column (4), we simultaneously control all the three-way interactions in the regression. The interaction with manufacturing intensity is statistically significant at the 1% level. Additionally, although the interactions with service and other industry intensity are also statistically significant, their magnitudes are almost negligible relative to the coefficient on the interaction with manufacturing intensity. These estimates indicate that the

[15]The industry division is based on the North American Industry Classification System (1997). Other industries include mining, utilities, and construction.

emission market did not influence housing markets through non-manufacturing industry intensities.

Other related characteristics. The correlations shown in Table A3.1 between manufacturing intensity and other county characteristics of age, education, and ethnicity raise a concern as to whether we are capturing the causal effect of the manufacturing intensity, or instead differential trends in house prices in less-educated, young, and high-minority areas. To address this problem, we directly control the interactions between $After \times NBP$ and other county characteristics. Notably, as shown in Table 3.6, the coefficient on $After \times NBP \times Manuf$ remains stable, while most coefficients on other three-way interactions are not statistically significantly different from zero.[16] We use the Wald test to check whether the summation of all other interactions is equal to zero. The *p-value* in columns (1) and (2) is 0.33 and 0.36, respectively, based on which we cannot reject the null hypothesis. By and large, including these controls does not influence the magnitude or significance of the coefficient on the manufacturing intensity.

3.5.4 Sensitivity analysis

NAAQS non-attainment status. In 2004, the EPA tightened the NAAQS ozone non-attainment standards, i.e., areas which did not satisfy the 1997-standard 8-hour ozone areas were designated. As a result, more than 400 counties were labelled as non-attainment

[16]The coefficient on $After \times NBP$ is no longer comparable to that in column (3) in Table 3.3. This coefficient in Table 3.6 represents the NBP effect on house prices for places with all characteristics equal to zero.

status. As we documented before, most of these non-attainment counties concentrated in the NBP regions. Additionally, a strand of literature has provided convincing evidence that the NAAQS attainment standards have negative impacts on labor markets. Therefore, the policy shock in 2004 may create different effects on house prices in the NBP regions. To tackle this concern, we add the interaction between the $After \times NBP$ and non-attainment status in 2004 into Equation (2).

Table 3.7 statistically summarizes the estimates based on the new specification. The coefficients on the three-way interaction with non-attainment status are negative and become statistically insignificant after we control the zip code-specific quadratic trends. More importantly, the coefficients on the manufacturing intensity interaction are less affected, compared to those in Table 3.3. The comparison demonstrates that including these controls in the regressions does not influence our main results much.

House types. In Panel A in Table 3.8, we replace the dependent variable with ZHVI for all home types. Since the NBP is supposed to have no influence on home sizes, we expect that the NBP impact on ZHVI should be similar to that on median price per square feet. The columns (1)-(4) replicate the specifications in Table 3.3. Column (3) indicates that house prices in areas with no manufacturing employment in 1998 increased by 12.23%. The three-way interaction shows that a 1% rise in manufacturing intensity in 1998 reduced house price increase by 0.045%. The magnitudes of the effects are similar to those in Table 3.3.

Next, we examine the NBP effects on single-family houses and condominium and cooperative homes, respectively. Both Panel B and C demonstrate that air pollution abatement resulted in a rise of house prices in low manufacturing intensity areas. However, house price growth was dampened in high manufacturing intensity NBP areas. These results suggest that the main results we derived are not driven by some particular house types.

Energy intensity. As Curtis (2014) pointed out, energy-intensity levels are different across manufacturing industries. The NBP may have limited effects on the labor market for areas with large proportion of low energy-intensity industries, e.g., electronic product manufacturing, beverage manufacturing, and etc. To take this issue into account, we construct another measurement of manufacturing intensity. Specifically, we assume that an area with L labor force has m and n workers in manufacturing industry M and N, respectively. The energy-intensity of the two industries are p and q, respectively.[17] Then for this area, the manufacturing intensity is computed as follow: $p \times \frac{m}{L} + q \times \frac{n}{L}$. The correlation between this newly-constructed measurement and the original one is about 0.53.

The estimates using this newly-constructed measurement are reported in Table 3.9. It demonstrates that our main conclusions do not change much. Specifically, as shown in column (3), house prices in districts with no manufacturing employment in 1998 increased by 12.79%, similar to that in Table 3.3. Additionally, the three-way interaction

[17]The energy expenditure data across industry are derived from the NBER Productivity Database. Following Curtis (2014), the energy-intensity is defined as the ratio between total industry energy expenditure and total value of shipments for one industry.

demonstrates that a 1% rise in manufacturing intensity in 1998 reduced house price increase by 0.038%, which is comparable to that in Table 3.3 as well.

Rust belt states. The rust belt states, including Illinois, Indiana, Michigan, Ohio, and Pennsylvania, have experienced economic decline and population loss for decades (e.g., Glaeser and Gyourko 2005). As these states are all in the NBP regions, their particularities may potentially drive our estimated treatment effects. In regressions, we already control zip code-specific linear and quadratic trends, which may partly address the concern. To further reduce the heterogeneity, we exclude these rust belt states from our regression sample. Table 3.10 presents the estimates based on the new sample. In column (3), the three-way interaction indicates that a 1% rise in manufacturing intensity in 1998 reduced house price increase by 0.058%, a slightly larger than that in Table 3.3. It is statistically significant at the 1% level. Therefore our main conclusions are insensitive to exclude rust belt states.

Adjacent states. As NO_x can be transported downwind to a long distance and thus the treatment status for states adjacent to the NBP regions is not obvious, they are excluded in our main analysis. To examine whether our results are robust to this step, we include these states as the control group. In Table A3.2, we present the estimates by assigning these states adjacent to NBP regions to the control group. The magnitude and significance of the coefficients become slightly smaller, compared to those in Table 3.3. Overall, the patterns of our main results are stable.

3.6 Conclusions

This paper has exploited both the air pollution and employment reductions induced by a major cap-and-trade market to provide new evidence on how housing markets are effected by environmental policies. Our difference-in-differences estimates indicate that house price growth in high manufacturing intensity areas in the participating states was dampened, whereas low manufacturing intensity regions experienced a housing boom. Specifically, we find that a 1% increase in manufacturing intensity in 1998 reduces house price increase by 0.046%. For perspective, house prices in an area with 40% manufacturing intensity in 1998 decreased by 4.63%, while house prices in an area with 5% manufacturing intensity in 1998 shifted up by 4.17%. These estimates are robust to a series of robustness tests.

From a efficiency view, cap-and-trade systems have their advantages compared to the command and control style regulations. Therefore, they are being widely adopted to tackle environmental problems, e.g., the Acid Rain Program, European Union's Emission Trading Scheme, Northeast Regional Greenhouse Gas Initiative, and so forth. But our paper shows that although emission markets provide a market-based solution to abate air pollution, they may also generate harmful side effects. Particularly, our findings present the first evidence of wealth redistribution as a results of environmental regulations. Given the ongoing academic and policy debates on energy sector regulations, comprehensively understanding the impacts of emission markets is highly relevant.

Tables and Figures of Chapter Three

Table 3.1: Descriptive statistics

Variables	(1) N	(2) Mean	(3) Std.Dev.	(4) Minimum	(5) Maximum
House prices (zip code level)					
Median home value per square feet (1,000$)	92,481	0.13	0.10	0.02	1.12
Home Value Index (1,000$)	90,281	219.47	186.74	24.15	3,809.13
Single-family HVI (1,000$)	89,810	230.78	209.68	24.15	3,840.52
Condo HVI (1,000$)	36,321	191.13	127.74	27.73	1,718.07
County characteristics					
The NBP region	92,481	0.62	0.49	0.00	1.00
Manufacturing employment (%)	92,481	17.36	9.41	0.00	61.59
Agricultural employment (%)	92,481	0.24	0.57	0.00	6.44
Service employment (%)	92,481	53.18	9.47	12.81	89.43
Other employment (%)	92,481	29.22	5.43	9.64	86.80
Median age	92,481	35.58	3.03	22.60	47.40
Bachelor's degree or higher (%)	92,481	25.92	9.57	5.60	60.20
College's degree (%)	92,481	27.67	4.72	11.80	41.80
High school's diploma (%)	92,481	28.54	7.37	11.70	51.10
Less than a High school's diploma (%)	92,481	17.87	6.37	3.00	56.60
White (%)	92,481	80.53	14.34	28.50	99.60
Black (%)	92,481	10.22	11.46	0.00	67.90
Asian (%)	92,481	4.00	4.60	0.10	32.60
Other races (%)	92,481	5.97	6.79	0.20	41.10

Note: Median home value per square feet is our main dependent variable in analysis. The corresponding sample includes 8,275 zip codes in 36 states. Other house value indices cover relative fewer areas. House prices or indexes are mean values in each zip code-year cell. The sample covers the period from 1998 through 2008.

Table 3.2: The overall impact of the NBP on house prices

	(1)	(2)	(3)
VARIABLES		Logged Median Price Per Sqr Ft	
After× NBP	-0.0609	0.0043	-0.0070
	(0.0852)	(0.0403)	(0.0404)
Observations	92,481	92,481	92,481
R-squared	0.9588	0.9878	0.9940
Zip Code FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes
Zip Code Quadratic Trend	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ is the differences-in-differences estimator, which equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 3.3: Impact by areas with different manufacturing intensities

	(1)	(2)	(3)	(4)
VARIABLES		Logged Median Price Per Sqr Ft		
After× NBP	0.2913***	0.1269**	0.1238**	
	(0.0959)	(0.0617)	(0.0599)	
After× NBP× Manuf	-0.1216***	-0.0430***	-0.0458***	-0.0396*
	(0.0290)	(0.0131)	(0.0166)	(0.0206)
After× Manuf	-0.0519**	-0.0246	-0.0151	-0.0259
	(0.0255)	(0.0146)	(0.0138)	(0.0184)
Observations	92,481	92,481	92,481	92,481
R-squared	0.9631	0.9879	0.9941	0.9945
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$ is the logged ratio between manufacturing employment and total labor force in each county in 1998. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 3.4: Robustness test: Non-parametric estimates

VARIABLES	(1)	(2)
	Logged Median Price Per Sqr Ft	
After× NBP× **1**(Manuf<=5%)	0.0619*	0.0634
	(0.0336)	(0.0449)
After× NBP× **1**(5%<Manuf<=10%)	0.0302	0.0168
	(0.0429)	(0.0433)
After× NBP× **1**(10%<Manuf<=15%)	0.0373	0.0266
	(0.0445)	(0.0404)
After× NBP× **1**(15%<Manuf<=40%)	-0.0178	-0.0290
	(0.0421)	(0.0428)
After× NBP× **1**(40%<Manuf<=45%)	-0.0248	-0.0271
	(0.0425)	(0.0557)
After× NBP× **1**(45%<Manuf<=50%)	-0.0319	-0.0476
	(0.0349)	(0.0465)
After× NBP× **1**(50%<Manuf)	-0.0518*	-0.0740*
	(0.0306)	(0.0374)
Observations	92,481	92,481
R-squared	0.9879	0.9941
Zip Code FE	Yes	Yes
Year FE	Yes	Yes
Zip Code Linear Trend	Yes	Yes
Zip Code Quadratic Trend	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). Due to the space limitation, twoway interactions between $After$ and manufacturing intensity groups are omitted. Ordinary least squares estimates. Standard errors in parentheses, clustered by state.

Table 3.5: Do non-manufacturing industries matter?

	(1)	(2)	(3)	(4)
VARIABLES	Logged Median Price Per Sqr Ft			
After× NBP	-0.0030	-0.0782	0.0512	
	(0.0385)	(0.0542)	(0.0608)	
After× NBP× Manuf				-0.0753***
				(0.0265)
After× Manuf				0.0156
				(0.2895)
After× NBP× Agriculture	-0.0232			-0.0076
	(0.0390)			(0.0369)
After× Agriculture	0.0181			0.0290
	(0.0299)			(0.2969)
After× NBP× Service		0.0013		0.0011*
		(0.0008)		(0.0006)
After× Service		0.0016**		-0.0060
		(0.0006)		(0.2768)
After× NBP× Others			-0.0013	0.0034**
			(0.0009)	(0.0013)
After× Others			-0.0017**	-0.0089
			(0.0007)	(0.2756)
Observations	92,481	92,481	92,481	92,481
R-squared	0.9940	0.9940	0.9940	0.9941
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	Yes	Yes	Yes	Yes
Zip Code Quadratic Trend	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$, $Agriculture$, $Service$, and $Others$ are the logged ratio between manufacturing, agricultural, service, and other employment and total labor force in each county in 1998, respectively. $Others$ includes mining, utilities, and construction employment. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 3.6: Other Population Characteristics

VARIABLES	(1)	(2)
	Logged Median Price Per Sqr Ft	
After× NBP× Manuf	-0.0388***	-0.0452***
	(0.0140)	(0.0163)
After× NBP× Median Age	0.1810	0.1765
	(0.1485)	(0.1760)
After× NBP× Bachelor and above	0.0488	0.0640
	(0.0767)	(0.0750)
After× NBP× College	0.1223	0.0377
	(0.0948)	(0.0745)
After× NBP× High School	-0.0165	0.0476
	(0.1226)	(0.1158)
After× NBP× Less than High School	-0.6517	-0.6353
	(0.7502)	(0.8137)
After× NBP× White	-0.0653	-0.0605
	(0.0760)	(0.0569)
After× NBP× Black	-0.0377**	-0.0418***
	(0.0139)	(0.0125)
After× NBP× Asian	-0.0127	0.0022
	(0.0149)	(0.0147)
Wald test (*p-value*)	0.33	0.36
Observations	92,481	92,481
R-squared	0.9886	0.9945
Zip Code FE	Yes	Yes
Year FE	Yes	Yes
Zip Code Linear Trend	Yes	Yes
Zip Code Quadratic Trend	No	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$ is the logged ratio between manufacturing employment and total labor force in each county in 1998. Due to the space limitation, twoway interactions between $After$ and other characteristics are omitted. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 3.7: NAAQS Regulations

VARIABLES	(1)	(2)	(3)	(4)
	Logged Median Price Per Sqr Ft			
After× NBP	0.3148***	0.1360***	0.1286*	
	(0.0949)	(0.0480)	(0.0638)	
After× NBP× Manuf	-0.1184***	-0.0450***	-0.0465**	-0.0351*
	(0.0277)	(0.0120)	(0.0177)	(0.0163)
After× Manuf	-0.0370*	-0.0096	-0.0029	-0.0246
	(0.0191)	(0.0090)	(0.0112)	(0.0175)
After× NBP× Non-att	-0.1187*	-0.0420*	-0.0338	-0.0041
	(0.0685)	(0.0242)	(0.0249)	(0.0190)
After× Non-att	0.2056***	0.1205***	0.0966***	0.0209***
	(0.0681)	(0.0318)	(0.0288)	(0.0061)
Observations	92,481	92,481	92,481	92,481
R-squared	0.9671	0.9884	0.9944	0.9946
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$ is the logged ratio between manufacturing employment and total labor force in each county in 1998. $Non-att$ is a dummy variable which equals to one if a county failed to meet the NAAQS ozone non-attainment standards in 2004 through 2008. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 3.8: Impacts by home type

	(1)	(2)	(3)	(4)
VARIABLES		**A. All Home Types**		
After× NBP	0.2744***	0.1262**	0.1223*	
	(0.0962)	(0.0621)	(0.0604)	
After× NBP× Manuf	-0.1170***	-0.0429***	-0.0454***	-0.0356*
	(0.0291)	(0.0134)	(0.0162)	(0.0202)
After× Manuf	-0.0519**	-0.0241	-0.0135	-0.0281
	(0.0250)	(0.0144)	(0.0135)	(0.0180)
Observations	90,281	90,281	90,281	90,281
R-squared	0.9606	0.9879	0.9941	0.9889
VARIABLES		**B. Single-family Houses**		
After× NBP	0.2590**	0.1206*	0.1177*	
	(0.0993)	(0.0634)	(0.0602)	
After× NBP× Manuf	-0.1114***	-0.0407***	-0.0436***	-0.0294
	(0.0292)	(0.0136)	(0.0157)	(0.0184)
After× Manuf	-0.0550**	-0.0248	-0.0143	-0.0317**
	(0.0240)	(0.0149)	(0.0140)	(0.0149)
Observations	89,810	89,810	89,810	89,810
R-squared	0.9723	0.9908	0.9954	0.9899
VARIABLES		**C. Condominium and Cooperative Homes**		
After× NBP	0.4233***	0.1634***	0.1763**	
	(0.1129)	(0.0459)	(0.0695)	
After× NBP× Manuf	-0.1728***	-0.0553***	-0.0671**	-0.0353
	(0.0570)	(0.0188)	(0.0252)	(0.0298)
After× Manuf	0.0070	-0.0092	-0.0018	-0.0232
	(0.0586)	(0.0262)	(0.0249)	(0.0327)
Observations	36,321	36,321	36,321	36,321
R-squared	0.9522	0.9837	0.9927	0.9838
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variables for Panel A, B, and C are the logged Zillow HVI for all home types, single-family HVI, and condo and cooperative home HVI, respectively. Standard errors in parentheses, clustered by state.

Table 3.9: An alternative measurement of manufacturing intensity

	(1)	(2)	(3)	(4)
VARIABLES	Logged Median Price Per Sqr Ft			
After× NBP	0.2887***	0.1325***	0.1279***	
	(0.0960)	(0.0273)	(0.0370)	
After× NBP× Manuf Energy	-0.0986***	-0.0363***	-0.0382***	-0.0208
	(0.0320)	(0.0106)	(0.0113)	(0.0145)
After× Manuf	0.0505*	0.0240*	0.0186	0.0058
	(0.0266)	(0.0141)	(0.0130)	(0.0093)
Observations	92,481	92,481	92,481	92,481
R-squared	0.9606	0.9879	0.9941	0.9865
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $ManufEnergy$ is the logged ratio between manufacturing employment and total labor force in each county in 1998, weighted by energy intensity of each industry. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table 3.10: Excluding rust belt states

VARIABLES	(1)	(2)	(3)	(4)
	Logged Median Price Per Sqr Ft			
After× NBP	0.3157***	0.1465**	0.1632***	
	(0.0844)	(0.0607)	(0.0569)	
After× NBP× Manuf	-0.1161***	-0.0434**	-0.0576***	-0.0513**
	(0.0349)	(0.0172)	(0.0188)	(0.0244)
After× Manuf	-0.0388	-0.0271	-0.0123	-0.0266
	(0.0326)	(0.0171)	(0.0176)	(0.0206)
Observations	72,868	72,868	72,868	72,868
R-squared	0.9632	0.9867	0.9936	0.9868
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). In this sample, we exclude the rust belt states, including Illinois, Indiana, Michigan, Ohio, and Pennsylvania. $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$ is the logged ratio between manufacturing employment and total labor force in each county in 1998. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

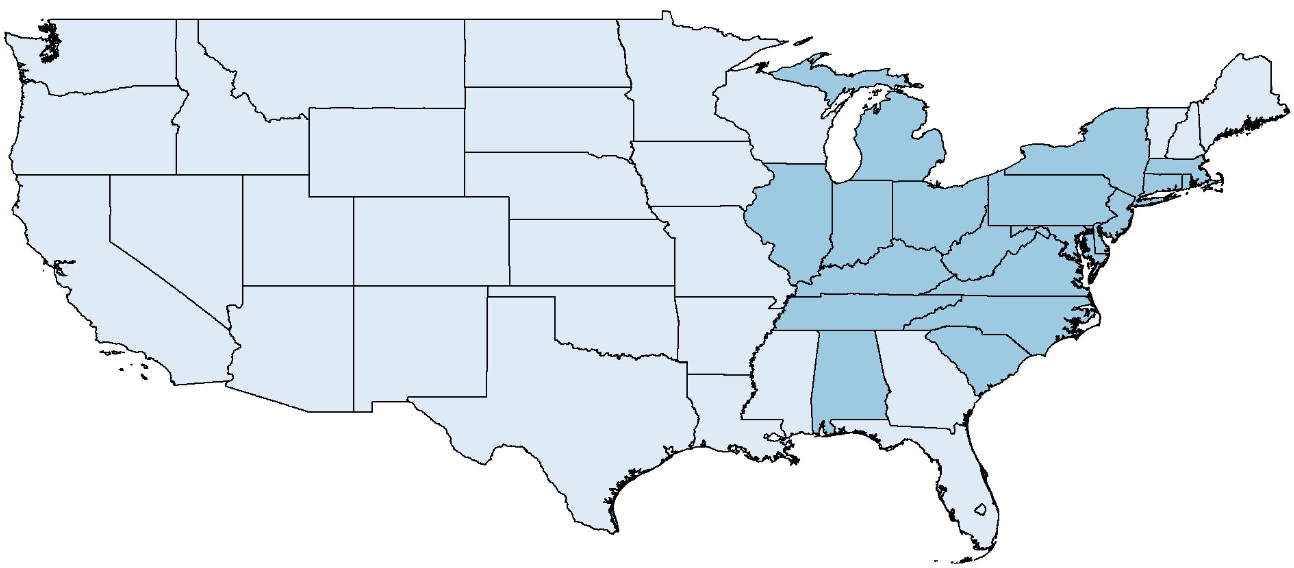
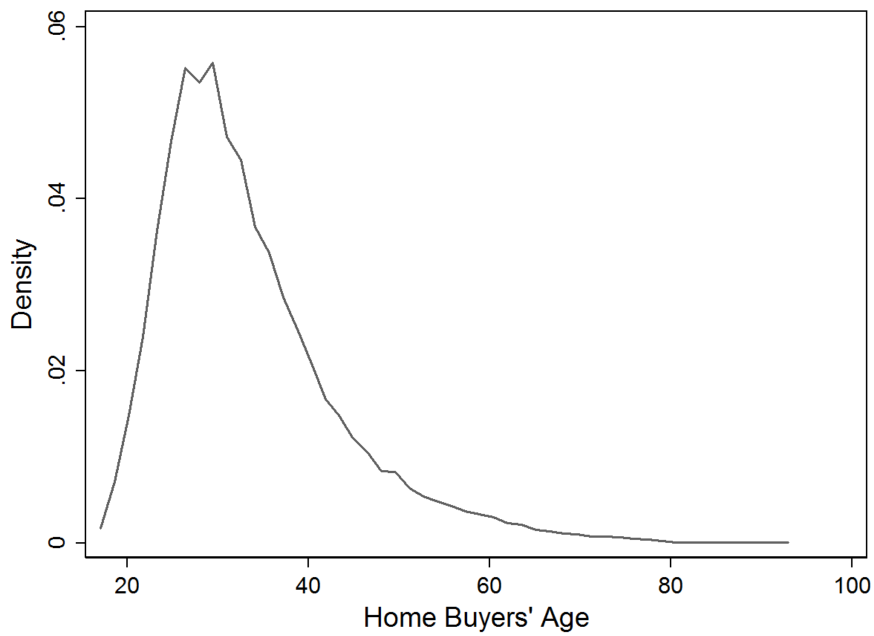
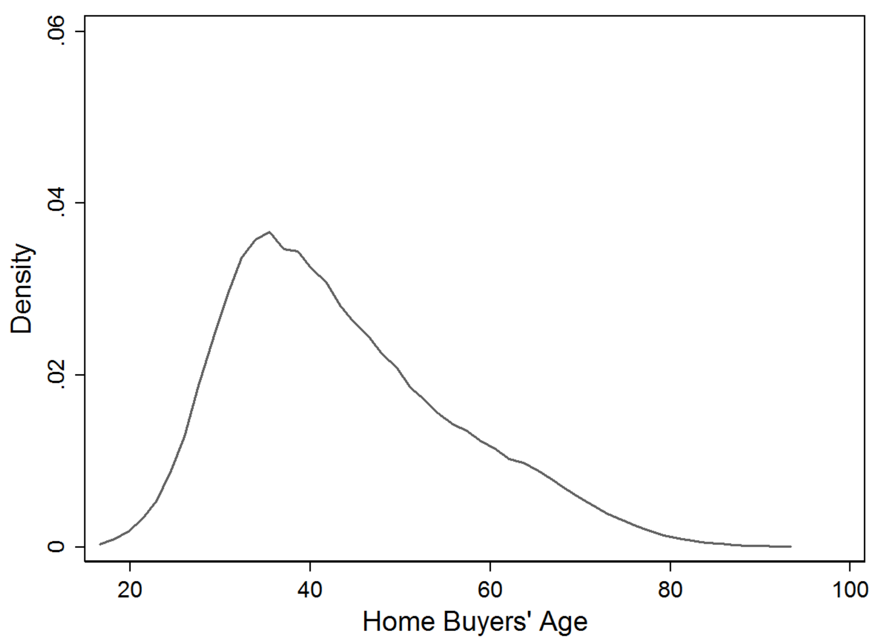


Figure 3.1: The states covered in the analysis sample

Note: Dark blue states are those participating in NBP during the 2003-2008 period (the NBP states). Light blue states are not participating (the non-NBP states). In analysis, we exclude the non-NBP states which are adjacent to NBP states, i.e., Arkansas, Florida, Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, Vermont, and Wisconsin. Non-continental states (Alaska and Hawaii) and Puerto Rico are not included as well. See details in data section.



(a) First-time Buyers



(b) Repeat Buyers

Figure 3.2: Home buyers' age distribution before the market's operation

Note: Based on data from the American Housing Survey (the national survey) in 1997, 1999, and 2001, the two figures display home buyers' age distribution before the emission market's initiation.

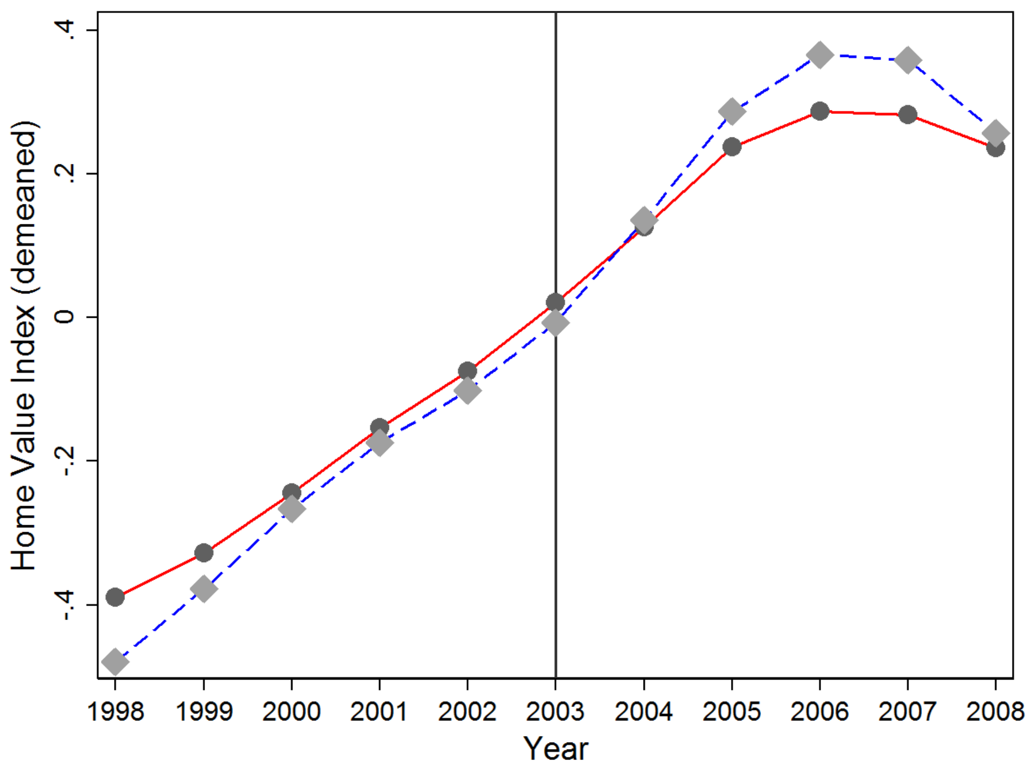


Figure 3.3: Average annual house prices in the NBP and non-NBP states during our sample period.

Note: The red solid line denotes the average median home value per square feet (demeaned) in the NBP regions in each year. The blue dash line represents the average median home value per square feet (demeaned) in the non-NBP regions in each year.

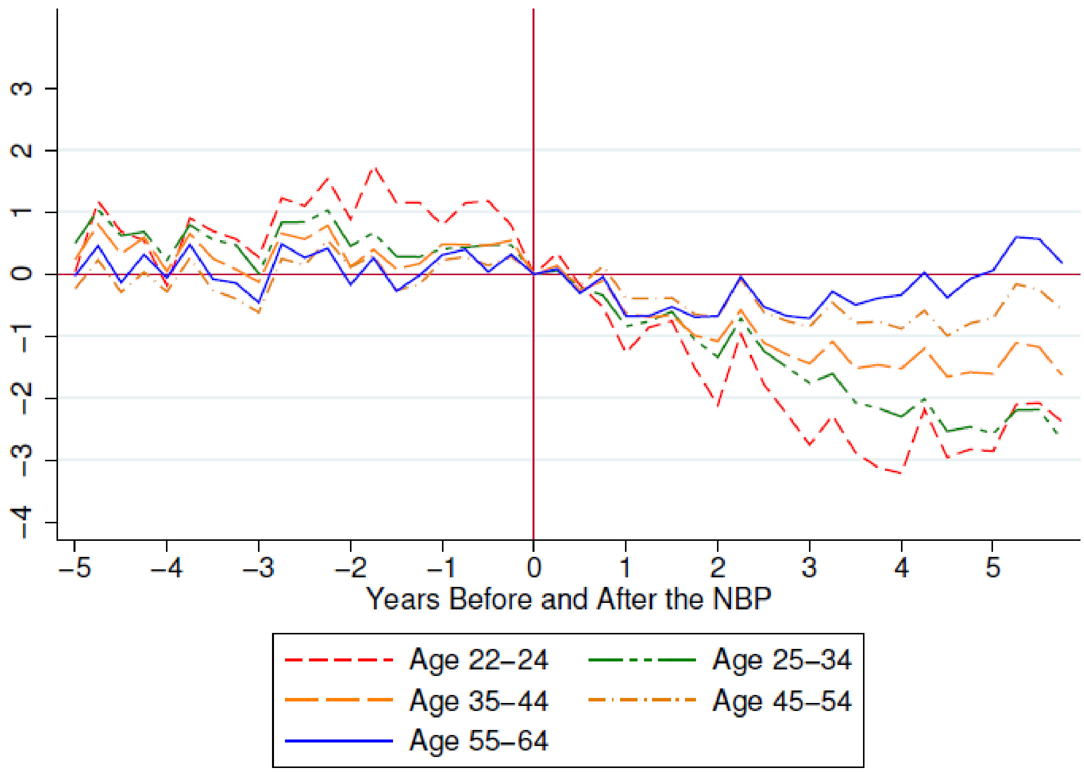
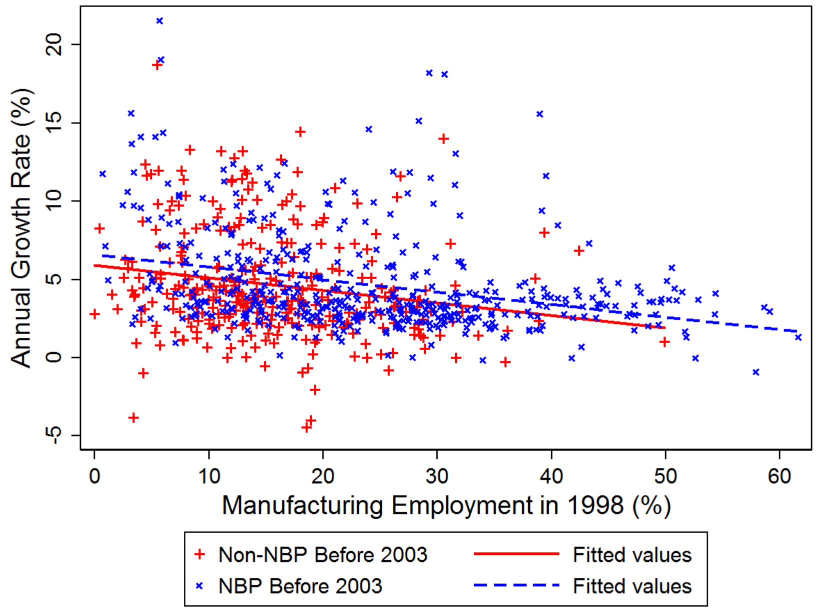
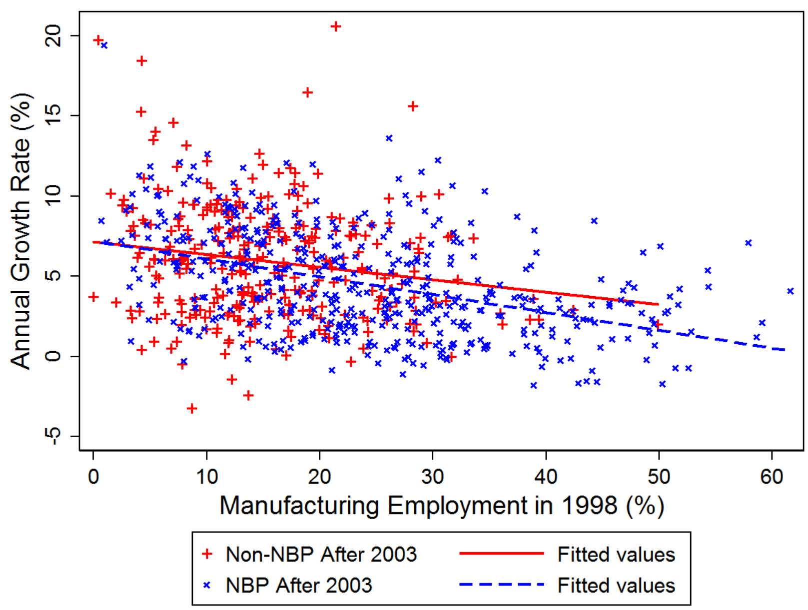


Figure 3.4: Impact of the NBP on employment by age group

Note: This figure is quoted from Curtis (2014). The y-axis denotes percent changes in employment. The plots are based on Equation (3) in Curtis (2014).



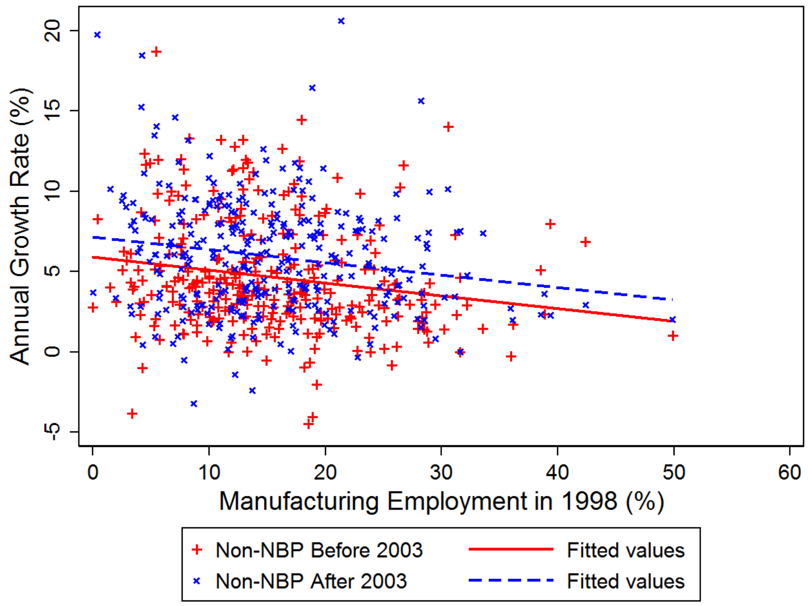
(a) NBP v.s. Non-NBP before 2003



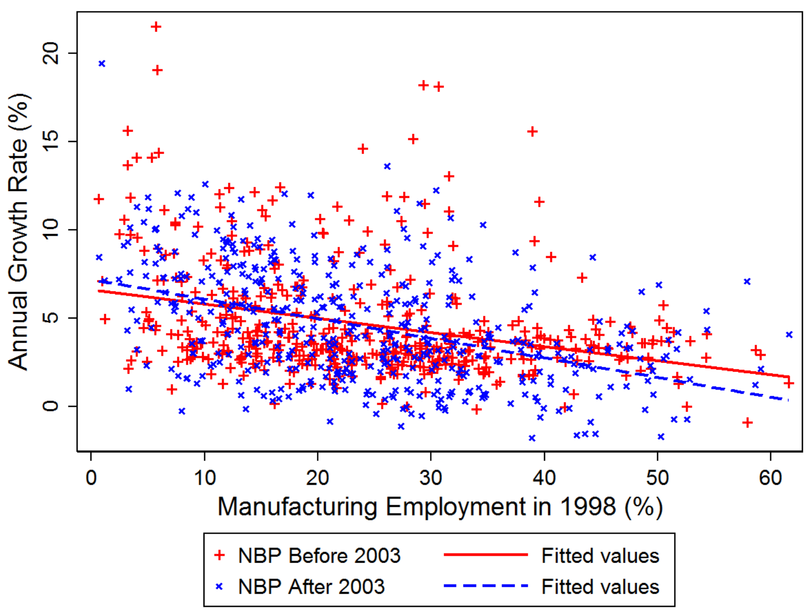
(b) NBP v.s. Non-NBP After 2003

Figure 3.5: Manufacturing employment (%) and house price growth rate (%)

Note: The y-axis in Panels (a) and (b) is average annual house price growth rate in each zip code area in 1998-2002 and 2003-2008, respectively. The x-axis denotes the ratio between manufacturing employment and total labor force in each county in 1998.



(a) Before 2003 v.s. After 2003 for Non-NBP



(b) Before 2003 v.s. After 2003 for NBP

Figure 3.6: Manufacturing employment (%) and house price growth rate (%)

Note: The y-axis in Panels (a) and (b) is average annual house price growth rate in each zip code area. The x-axis denotes the ratio between manufacturing employment and total labor force in each county in 1998.

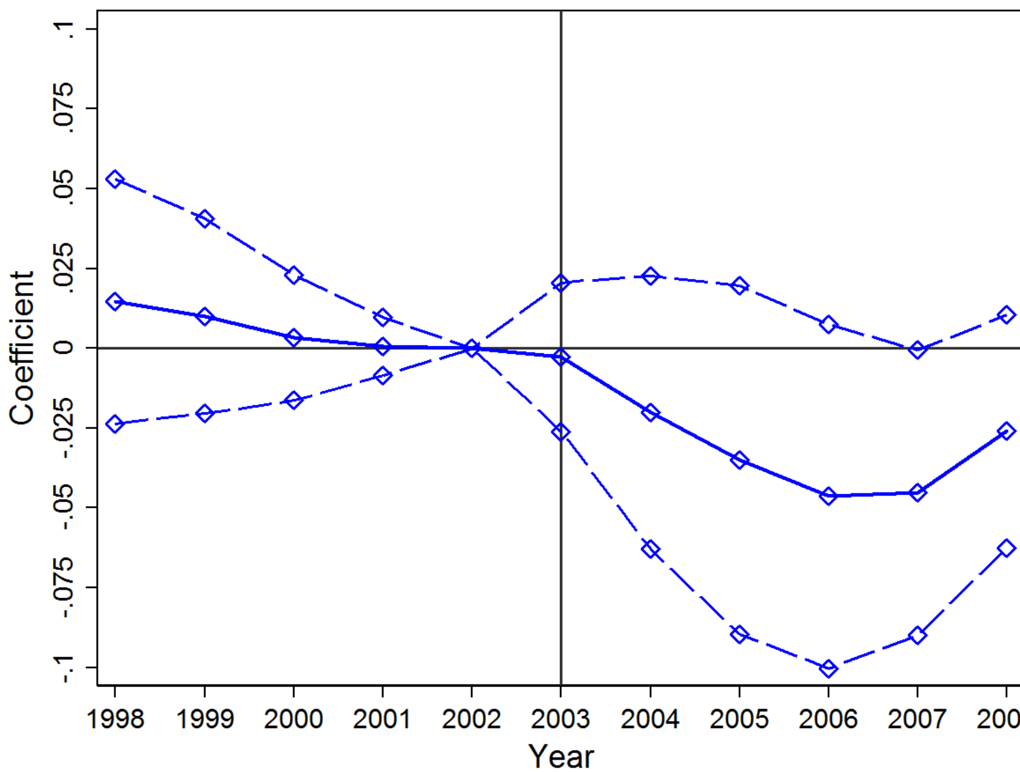


Figure 3.7: The NBP effect across year

Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.

Appendix of Chapter Three

Table A3.1: Correlations between county characteristics

VARIABLES	Manufacturing employment (%)
Median age	-0.0966
Bachelor's degree or higher (%)	-0.2810
College's degree (%)	-0.3319
High school's diploma (%)	0.3351
Less than a High school's diploma (%)	0.2227
White (%)	-0.0015
Black (%)	0.1669
Asian (%)	-0.0973
Other races (%)	-0.2295

Table A3.2: Robustness test: Assigning adjacent states to the control group

	(1)	(2)	(3)	(4)
VARIABLES	Logged Median Price Per Sqr Ft			
After× NBP	0.1769	0.1010*	0.0874	
	(0.1093)	(0.0535)	(0.0606)	
After× NBP× Manuf	-0.0845**	-0.0386***	-0.0359**	-0.0387*
	(0.0347)	(0.0134)	(0.0175)	(0.0177)
After× Manuf	-0.1007***	-0.0370**	-0.0333*	-0.0211
	(0.0319)	(0.0159)	(0.0169)	(0.0151)
Observations	116,048	116,048	116,048	116,048
R-squared	0.9601	0.9868	0.9930	0.9858
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. In this sample, states adjacent to the NBP states are assigned the control group, respectively. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$ is the logged ratio between manufacturing employment and total labor force in each county in 1998. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table A3.3: Robustness test: Manufacturing intensity between 1998 and 2002

VARIABLES	(1)	(2)	(3)	(4)
	Logged Median Price Per Sqr Ft			
After× NBP	0.2450***	0.1097*	0.1065*	
	(0.0893)	(0.0561)	(0.0533)	
After× NBP× Manuf	-0.1113***	-0.0390***	-0.0419***	-0.0413*
	(0.0273)	(0.0118)	(0.0148)	(0.0143)
After× Manuf	-0.0467*	-0.0216	-0.0127	-0.0199
	(0.0246)	(0.0133)	(0.0132)	(0.0192)
Observations	92,481	92,481	92,481	92,481
R-squared	0.9633	0.9880	0.9941	0.9869
Zip Code FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Zip Code Linear Trend	No	Yes	Yes	No
Zip Code Quadratic Trend	No	No	Yes	No
State-Year FE	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the median home value per square feet for all home types (in logarithm). $After \times NBP$ equals to 1 for all zip codes belonging to the NBP states in 2003 (or 2004) through 2008. $Manuf$ is the average logged ratio between manufacturing employment and total labor force in each county between 1998 and 2002. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state.

Table A3.4: The NBP effect on NO_x emissions

	(1)	(2)
VARIABLES	Logged NO_x emissions in summertime	
After× NBP	-0.1734***	-0.1679
	(0.0439)	(0.1417)
After× NBP× Manuf		-0.0021
		(0.0398)
After× Manuf		0.012
		(0.025)
Observations	6,139	6,139
R-squared	0.9816	0.9817
County FE	Yes	Yes
Year FE	Yes	Yes
County Linear Trend	Yes	Yes
County Quadratic Trend	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the total NO_x emissions in summertime in each county-year cell (in logarithm). Ordinary least squares estimates. Standard errors in parentheses, clustered by state.

Bibliography

Almond, D. (2006). Is the 1918 influenza pandemic over? Long term effects of in utero influenza exposure in the post 1940 U.S. population. *Journal of Political Economy*, 114(4):672–712.

Almond, D., Edlund, L., and Palme, M. (2009). Chernobyl's subclinical legacy: prenatal exposure to radioactive fallout and school outcomes in sweden. *Quarterly Journal of Economics*, 124(4):1729–1772.

Almond, D. and Mazumder, B. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. *American Economic Journal: Applied Economics*, 3(4):56–85.

Barreca, A., Clay, K., Deschenes, O., Greenstone, M., and Shapiro, J. S. (2016). Adapting to climate change: the remarkable decline in the us temperature-mortality relationship over the twentieth century. *Journal of Political Economy*, 124(1):105–159.

Barreca, A., Deschenes, O., and Guldi, M. (2015). Maybe next month? Temperature shocks, climate change, and dynamic adjustments in birth rates. *NBER Working Paper 21681*.

Bayer, P., Keohane, N., and Timmins, C. (2009). Migration and hedonic valuation: The case of air quality. *Journal of Environmental Economics and Management*, 58(1):1–14.

Behrman, J. R. and Rosenzweig, M. R. (2004). Returns to birthweight. *Review of Economics and statistics*, 86(2):586–601.

Berry, H. L., Bowen, K., and Kjellstrom, T. (2010). Climate change and mental health: a causal pathways framework. *International Journal of Public Health*, 55(2):123–132.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2005). The more the merrier? the effect of family size and birth order on children's education. *The Quarterly Journal of Economics*, pages 669–700.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2007). From the cradle to the labor market? the effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, 122(1):409–439.

Block, M. L. and Calderón-Garcidueñas, L. (2009). Air pollution: mechanisms of neuroinflammation and CNS disease. *Trends in Neurosciences*, 32(9):506–516.

Brooks, N., Adger, W. N., and Kelly, P. M. (2005). The determinants of vulnerability and adaptive capacity at the national level and the implications for adaptation. *Global Environmental Change*, 15(2):151–163.

Buckles, K. S. and Hungerman, D. M. (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics*, 95(3):711–724.

Burgess, R., Deschenes, O., Donaldson, D., and Greenstone, M. (2011). Weather and death in India. *Cambridge, United States: Massachusetts Institute of Technology, Department of Economics. Manuscript*.

Buxton, L. D. (2013). *The peoples of Asia*. Routledge.

Campbell, J. M. (1983). Ambient stressors. *Environment and Behavior*, 15(3):355–380.

Carrillo, P. E., Fishman, R., and Russ, J. (2015). Long-term impacts of high temperatures on economic productivity. *Unpublished Manuscript*.

Chalfin, A. and McCrary, J. (2013). Are US cities under-policed? theory and evidence. *NBER Working Paper*, 18815.

Chay, K. Y. and Greenstone, M. (2003). The impact of air pollution on infant mortality: Evidence from geographic variation in pollution shocks induced by a recession. *The Quarterly Journal of Economics*, 118(3):1121–1167.

Chay, K. Y. and Greenstone, M. (2005). Does air quality matter? evidence from the housing market. *Journal of Political Economy*, 113(2):376–424.

Chen, Y., Ebenstein, A., Greenstone, M., and Li, H. (2013). Evidence on the impact of sustained exposure to air pollution on life expectancy from Chinas Huai River policy. *Proceedings of the National Academy of Sciences*, 110(32):12936–12941.

Chen, Y. and Zhou, L.-A. (2007). The long-term health and economic consequences of the 1959–1961 famine in China. *Journal of Health Economics*, 26(4):659–681.

Chuang, K.-J., Chan, C.-C., Su, T.-C., Lee, C.-T., and Tang, C.-S. (2007). The effect of urban air pollution on inflammation, oxidative stress, coagulation, and autonomic dysfunction in young adults. *American Journal of Respiratory and Critical Care Medicine*, 176(4):370–376.

Corradi, M., Alinovi, R., Goldoni, M., Vettori, M. V., Folesani, G., Mozzoni, P., Cavazzini, S., Bergamaschi, E., Rossi, L., and Mutti, A. (2002). Biomarkers of oxidative stress after controlled human exposure to ozone. *Toxicology Letters*, 134(1):219–225.

Currie, J. and Almond, D. (2011). Human capital development before age five. *Handbook of Labor Economics*, 4:1315–1486.

Currie, J., Davis, L., Greenstone, M., and Walker, R. (2015). Environmental health risks and housing values: Evidence from 1,600 toxic plant openings and closings. *The American Economic Review*, 105(2):678–709.

Currie, J. and Neidell, M. (2005). Air pollution and infant health: What can we learn from california's recent experience? *The Quarterly Journal of Economics*, pages 1003–1030.

Currie, J. and Rossin-Slater, M. (2013). Weathering the storm: Hurricanes and birth outcomes. *Journal of Health Economics*, 32(3):487–503.

Curtis, E. M. (2014). Who loses under power plant cap-and-trade programs? *NBER Working Paper 20808*.

Deschenes, O., Greenstone, M., and Guryan, J. (2009). Climate change and birth weight. *The American Economic Review*, 99(2):211–217.

Deschenes, O., Greenstone, M., and Shapiro, J. S. (2012). Defensive investments and the demand for air quality: Evidence from the NOx Budget Program and ozone reductions. *NBER Working Paper 18267*.

Duncan, B., Mansour, H., and Rees, D. I. (2016). It's just a game: the super bowl and low birth weight. *Journal of Human Resources*, pages 0615–7213R.

Ebenstein, A., Lavy, V., and Roth, S. (2016). The long run economic consequences of high-stakes examinations: evidence from transitory variation in pollution. *American Economic Journal: Applied Economics*.

Elter, K., Ay, E., Uyar, E., and Kavak, Z. N. (2004). Exposure to low outdoor temperature in the midtrimester is associated with low birth weight. *Australian and New Zealand Journal of Obstetrics and Gynaecology*, 44(6):553–557.

Engle, R. F., Granger, C. W., Rice, J., and Weiss, A. (1986). Semiparametric estimates of the relation between weather and electricity sales. *Journal of the American Statistical Association*, 81(394):310–320.

Evans, G. W., Jacobs, S. V., Dooley, D., and Catalano, R. (1987). The interaction of stressful life events and chronic strains on community mental health. *American Journal of Community Psychology*, 15(1):23–34.

Federal Bureau of Investigation (2011). Crime in the United States, 2010: About the Uniform Crime Reporting program.

Feng, S., Krueger, A. B., and Oppenheimer, M. (2010). Linkages among climate change, crop yields and Mexico–US cross-border migration. *Proceedings of the National Academy of Sciences*, 107(32):14257–14262.

Figlio, D., Hamersma, S., and Roth, J. (2009). Does prenatal wic participation improve birth outcomes? new evidence from florida. *Journal of Public Economics*, 93(1):235–245.

Fowlie, M. (2010). Emissions trading, electricity restructuring, and investment in pollution abatement. *The American Economic Review*, 100(3):837–869.

Fowlie, M., Knittel, C. R., and Wolfram, C. (2012). Sacred cars? optimal regulation of stationary and non-stationary pollution sources. *American Economic Journal: Economic Policy*, 4(1):98–126.

Glaeser, E. L. and Gyourko, J. (2005). Urban decline and durable housing. *Journal of Political Economy*, 113(2):345–375.

Gould, E. D., Weinberg, B. A., and Mustard, D. B. (2002). Crime rates and local labor market opportunities in the United States: 1979–1997. *Review of Economics and Statistics*, 84(1):45–61.

Greenstone, M. (2002). The impacts of environmental regulations on industrial activity: Evidence from the 1970 and 1977 clean air act amendments and the census of manufactures. *Journal of Political Economy*, 110(6):1175–1219.

Greenstone, M., List, J. A., and Syverson, C. (2012). The effects of environmental regulation on the competitiveness of us manufacturing. *NBER Working Paper 18392*.

Guerrieri, V., Hartley, D., and Hurst, E. (2013). Endogenous gentrification and housing price dynamics. *Journal of Public Economics*, 100:45–60.

Hanna, R. and Oliva, P. (2015). The effect of pollution on labor supply: Evidence from a natural experiment in mexico city. *Journal of Public Economics*, 122:68–79.

Hansen, A., Bi, P., Nitschke, M., Ryan, P., Pisaniello, D., and Tucker, G. (2008). The effect of heat waves on mental health in a temperate australian city. *Environmental Medicine*, 116:1369–1375.

Herman, C. P. (1993). Effects of heat on appetite. *Nutritional needs in hot environments: applications for military personnel in field operations*, pages 187–214.

Herrnstadt, E., Heyes, A., Muehlegger, E., and Saberian, S. (2016). Air pollution as a cause of violent crime: Evidence from Los Angeles and Chicago.

Hollinger, S. and Angel, J. (2009). Illinois agronomy handbook. *Urbana, IL: University of Illinois Extension*, page 224.

Intergovernmental Panel on Climate Change (2013). Climate change 2013: The physical science basis.

Isen, A., Rossin-Slater, M., and Walker, W. R. (2015). Heat and long-run human capital formation. *Unpublished Manuscript*.

Jacob, B., Lefgren, L., and Moretti, E. (2007). The dynamics of criminal behavior evidence from weather shocks. *Journal of Human Resources*, 42(3):489–527.

Jones, J. W. and Bogat, G. A. (1978). Air pollution and human aggression. *Psychological Reports*.

Kahn, M. E. and Mansur, E. T. (2013). Do local energy prices and regulation affect the geographic concentration of employment? *Journal of Public Economics*, 101:105–114.

Kim, C. W., Phipps, T. T., and Anselin, L. (2003). Measuring the benefits of air quality improvement: A spatial hedonic approach. *Journal of Environmental Economics and Management*, 45(1):24–39.

Lave, L. and Seskin, E. (1978). *Air pollution and human health*. Baltimore.

Levesque, S., Surace, M. J., McDonald, J., and Block, M. L. (2011). Air pollution & the brain: subchronic diesel exhaust exposure causes neuroinflammation and elevates early markers of neurodegenerative disease. *Journal of Neuroinflammation*, 8(1):1.

Linn, J. (2008). Technological modifications in the nitrogen oxides tradable permit program. *The Energy Journal*, pages 153–176.

Linn, J. (2010). The effect of cap-and-trade programs on firms profits: Evidence from the nitrogen oxides budget trading program. *Journal of Environmental Economics and Management*, 59(1):1–14.

Luechinger, S. (2009). Valuing air quality using the life satisfaction approach. *The Economic Journal*, 119(536):482–515.

Maccini, S. and Yang, D. (2009). Under the weather: Health, schooling, and economic consequences of early-life rainfall. *The American Economic Review*, 99(3):1006–1026.

McCollister, K. E., French, M. T., and Fang, H. (2010). The cost of crime to society: New crime-specific estimates for policy and program evaluation. *Drug and alcohol dependence*, 108(1):98–109.

McDowall, D., Loftin, C., and Pate, M. (2012). Seasonal cycles in crime, and their variability. *Journal of Quantitative Criminology*, 28(3):389–410.

Mian, A. and Sufi, A. (2009). The consequences of mortgage credit expansion: Evidence from the us mortgage default crisis. *The Quarterly Journal of Economics*, 124(4):1449–1496.

Miron, J. A. (1996). *The economics of seasonal cycles*. MIT Press.

Murray, L. J., O'reilly, D., Betts, N., Patterson, C., Smith, G. D., and Evans, A. (2000). Season and outdoor ambient temperature: Effects on birth weight. *Obstetrics & Gynecology*, 96(5, Part 1):689–695.

Nilsson, P. (2014). Alcohol availability, prenatal conditions, and long-term economic outcomes. *Institute for International Economic Studies, Stockholm University, unpublished manuscript*.

Patel, R. R., Steer, P., Doyle, P., Little, M. P., and Elliott, P. (2004). Does gestation vary by ethnic group? A London-based study of over 122 000 pregnancies with spontaneous onset of labour. *International Journal of Epidemiology*, 33(1):107–113.

Persson, P. and Rossin-Slater, M. (2016). Family ruptures, stress, and the mental health of the next generation. *American Economic Review*.

Peterman, A., Ng, S. W., Palermo, T., and Lee, I.-H. E. (2013). Managing the double burden: Effects of pregnancy on labor-intensive time use in rural china, mexico and tanzania. *Studies in family planning*, 44(4):411.

Rammal, H., Bouayed, J., Younos, C., and Soulimani, R. (2008). Evidence that oxidative stress is linked to anxiety-related behaviour in mice. *Brain, Behavior, and Immunity*, 22(8):1156–1159.

Ranson, M. (2014). Crime, weather, and climate change. *Journal of Environmental Economics and Management*, 67(3):274–302.

Raphael, S. and Winter-Ebmer, R. (2001). Identifying the effect of unemployment on crime. *The Journal of Law and Economics*, 44(1):259–283.

Roback, J. (1982). Wages, rents, and the quality of life. *Journal of Political Economy*, 90(6):1257–1278.

Robinson, P. M. (1988). Root-n-consistent semiparametric regression. *Econometrica*, 56(4):931–954.

Rosen, S. (1974). Hedonic prices and implicit markets: Product differentiation in pure competition. *Journal of Political Economy*, 82(1):34–55.

Rotton, J. (1983). Affective and cognitive consequences of malodorous pollution. *Basic and Applied Social Psychology*, 4(2):171–191.

Rotton, J. and Frey, J. (1985). Air pollution, weather, and violent crimes: concomitant time-series analysis of archival data. *Journal of Personality and Social Psychology*, 49(5):1207.

Rotton, J., Frey, J., Barry, T., Milligan, M., and Fitzpatrick, M. (1979). The air pollution experience and physical aggression. *Journal of Applied Social Psychology*, 9(5):397–412.

Schifano, P., Lallo, A., Asta, F., De Sario, M., Davoli, M., and Michelozzi, P. (2013). Effect of ambient temperature and air pollutants on the risk of preterm birth, rome 2001–2010. *Environment international*, 61:77–87.

Schlenker, W. and Roberts, M. J. (2009). Nonlinear temperature effects indicate severe damages to US crop yields under climate change. *Proceedings of the National Academy of Sciences*, 106(37):15594–15598.

Schlenker, W. and Walker, W. R. (2015). Airports, air pollution, and contemporaneous health. *The Review of Economic Studies*, page rdv043.

Shapiro, J. S. and Walker, R. (2015). Why is pollution from us manufacturing declining? the roles of trade, regulation, productivity, and preferences. *NBER Working Paper 20879*.

Strahilevitz, M., Strahilevitz, A., and Miller, J. E. (1979). Air pollutants and the admission rate of psychiatric patients. *Am J Psychiatry*, 136(2):205–7.

Strand, L. B., Barnett, A. G., and Tong, S. (2011). The influence of season and ambient temperature on birth outcomes: A review of the epidemiological literature. *Environmental Research*, 111(3):451–462.

Streets, D., Chang, Y.-S., Tompkins, M., Ghim, Y., and Carter, L. (2001). Efficient regional ozone control strategies for the eastern united states. *Journal of Environmental Management*, 61(4):345–365.

USEPA (2009). The nox budget trading program: 2008 highlights. *Washington, DC: USEPA*.

Valavanidis, A., Vlachogianni, T., Fiotakis, K., and Loridas, S. (2013). Pulmonary oxidative stress, inflammation and cancer: respirable particulate matter, fibrous dusts and ozone as major causes of lung carcinogenesis through reactive oxygen species mechanisms. *International Journal of Environmental Research and Public Health*, 10(9):3886–3907.

Wacziarg, R. and Spolaore, E. (2013). How deep are the roots of economic development? *Journal of Economic Literature*, 51(2):325.

Walker, W. R. (2011). Environmental regulation and labor reallocation: Evidence from the clean air act. *The American Economic Review*, 101(3):442–447.

Walker, W. R. (2013). The transitional costs of sectoral reallocation: Evidence from the clean air act and the workforce. *The Quarterly Journal of Economics*, 128(4):1787–1835.

Wilde, J., Apouey, B., Jung, T., et al. (2016). The effect of ambient temperature shocks during conception and early pregnancy on later life outcomes. *Unpublished Manuscript*.

Xie, Y. (2012). The user's guide of the China Family Panel Studies (2010). *Beijing: Institute of Social Science Survey, Peking University*.

Zeidner, M. and Shechter, M. (1988). Psychological responses to air pollution: Some personality and demographic correlates. *Journal of Environmental Psychology*, 8(3):191–208.

Zheng, S., Kahn, M. E., and Liu, H. (2010). Towards a system of open cities in china: Home prices, fdi flows and air quality in 35 major cities. *Regional Science and Urban Economics*, 40(1):1–10.

Zivin, J. G. and Neidell, M. (2012). The impact of pollution on worker productivity. *The American Economic Review*, 102(7):3652–3673.

Zivin, J. S. G., Hsiang, S. M., and Neidell, M. J. (2015). Temperature and human capital in the short-and long-run. *NBER Working Paper 21157*.